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PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

SIR WILLIAM THOMSON, KNT. LL.D. F.R.S. &c.

AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. l. Not.*

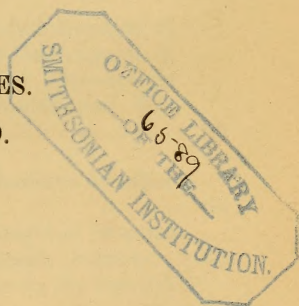
VOL. VII.—FIFTH SERIES.

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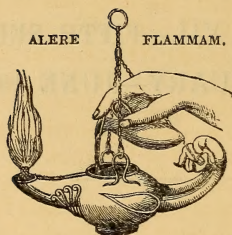
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"Meditationis est perscrutari occulta; contemplationis est admirari
perspicua Admiratio generat quæstionem, quæstio investigationem,
investigatio inventionem."—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat
Cur mare turgescat, pelago cur tantus amaror
Cur caput obscura Phœbus ferrugine condat
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina coelo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.



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THE
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[FIFTH SERIES.]

JANUARY 1879.

I. *On the Excitation of Electricity at the Contact of Solids and Gases.* By W. BEETZ*.

WHEN publishing my first experiments on the electromotive forces of gas batteries, I expressed my ideas respecting the place at which the seat of the difference of tension produced was to be sought†. Grove had assumed that it was the place of contact of the platinum, gas, and liquid‡. I did not admit the universal correctness of that assumption: it is certainly not true for gases which, like chlorine, strongly absorb water; for a platinum plate entirely immersed in a liquid containing chlorine behaves, in respect of its electricity, very differently from a plate of platinum immersed in a liquid free from chlorine. I showed that what happens with other gases may be regarded as precisely similar; only it is the less distinctly manifested the less soluble they are in the liquid. The upper part of a platinum plate, enveloped in hydrogen, I covered with an insulating layer, so that the free platinum was entirely covered by the liquid, and yet it preserved a real gaseous element, certainly of somewhat less electromotive force than if the upper end of the platinum had been directly surrounded by the gas. I have in the place above cited given my views respecting the reasons for this difference. Gauguain subsequently arrived at the conclusion that the platinum acts only upon the gases dissolved in the liquid§. A platinum

* Translated from Wiedemann's *Annalen*, 1878, No. 9, vol. v. pp. 1-20.

† Pogg. *Ann.* vol. lxxvii. p. 505. ‡ Phil. *Trans.* 1813, pt. 2, p. 97.

§ *Comptes Rendus*, t. lxiv. p. 364 (1867).

wire surrounded by the gas and dipping in the liquid he gradually lowered until it was all covered by the latter; he then obtained precisely the same difference of tension as when one part of the wire was surrounded by the gas and the other by the liquid. This result I accounted for by remarking that, in making the experiment thus, the wire had at first been actually in contact with the gas, and then carried a condensed layer of gas with it into the liquid*. I have further, in the above-mentioned treatises, stated my views upon the following:—that the amount of the difference of tension between a clean metal and one coated with a gas depends on the degree of such condensation of the gases; that the condensation is greater or less, according to the metal with which the gas elements have been produced; and that a singularly high degree of condensation is produced by electrolytic polarization, on account of which the electromotive force of the gases is in this case peculiarly great. The considerable difference of tension produced by the action of even small quantities of hydrogen on platinum I compared to the analogous phenomenon shown by the position of the amalgams in the tension series. Macaluso has moreover pointed out that far greater electromotive forces can be generated by the long-continued electrolytic evolution of hydrogen, or chlorine, or at platinum or carbon electrodes than by simple contact of the gases with the plates or by gas being evolved at them during a short time; he therefore believed that an active state must be ascribed to the gases separated by electrolysis, similar to that which is known to us in oxygen†. In truth, as regards hydrogen, the presence of an active modification, previously assumed by Osann, has been rendered very doubtful by Magnus‡.

While the subject of all the above investigations was the presence of considerable quantities of gas on the metal plates, there has recently been a discussion in detail of the case in which only thin films of gas have formed upon the plates. F. Kohlrausch has subjected these films to a careful consideration§; and Helmholtz|| and Herwig¶ have made the analogy between a layer of liquid connecting two polarized electrodes and a condenser the subject of their investigations. Herein Helmholtz has advocated the view that in the polari-

* Pogg. *Ann.* vol. cxxxii. p. 461.

† *Ber. d. k. sächs. Ges. d. Wiss., math.-phys. Cl.* 1873, p. 306.

‡ *Conf. Wiedemann, Galvanismus*, 2nd ed. vol. i. p. 533.

§ *Gött. Nachr.* 1872, No. 23, p. 453.

|| *Monatsb. d. Berl. Akad. d. Wiss.* 1873, p. 587.

¶ *Wied. Ann.* ii. p. 566.

zation not merely portions of gas adhering to the surface, but also portions which have penetrated deeper into the platinum must play a part—the possibility of which had been already indicated by Graham's experiments on palladium and platinum. In fact, Crova*, and after him Root also †, succeeded in proving that, with the electrolysis of dilute sulphuric acid, hydrogen penetrates through a platinum plate, since the plate not only exhibited polarization on the side where the electrolysis took place, but was protected on the opposite side from all electrolytic action.

But few investigators, in studying galvanic polarization, have taken into consideration other gases than hydrogen and oxygen; hence the question arises whether the views which hold good for these two gases, and preferably for hydrogen, can be extended in their entirety to all cases of polarization. A series of experiments which I have made with palladium and carbon electrodes may contribute to the answering of this question.

To procure exact knowledge respecting the electromotive position of palladium is a very difficult task. Palladium as obtained in commerce has always been heated to incandescence, and has taken in gases in the process, as Graham has shown. The means usually employed to expel such occluded gases, especially hydrogen, from the palladium are so far sufficient that chemical analysis can show no traces remaining, but do not suffice to destroy all change in the electromotive state of the metal. This is especially true of the treatment with the mercury air-pump; I have never been able in this way to bring back a palladium plate quite to its previous electromotive position after evolution of hydrogen had taken place on it. On the other hand, the last trace of hydrogen can be completely removed by a long-continued evolution of oxygen. But then the plate becomes covered with a coat of brown oxide; and if this be ever so carefully rubbed off, yet the plate always takes a much more negative position in the tension series than if it had been cleaned with hydrochloric acid. For the determination of that position I made use of my universal compensator‡, with which also all the other measurements of the differences of tension we shall have to consider were made. The palladium plate to be tried dipped into very dilute sulphuric acid (1:100), and thus formed the negative constituent of an element, of which the positive consisted of an amalgamated zinc cylinder standing in a concen-

* *Mondes T. V.* p. 210 (1864); *Wied. Galv.* (2) i. § 498.

† *Monatsber. d. Berl. Akad. d. Wiss.* 1876, p. 217.

‡ *Wied. Ann.* iii. p. 1.

trated solution of sulphate of zinc. The two fluids were connected with one another by means of a siphon filled with dilute sulphuric acid and closed at both ends by clay cells. A Daniell element in the form previously employed by me served as unit, its zinc-cell being filled with solution of sulphate of zinc. If d denote electromotive force of such an element, and D that of a Daniell element the zinc-cell of which contains dilute sulphuric acid, then $d=0.95D$. As the force D is generally taken for the unit force, I have reduced all the following data to the same. In like manner I cite from previous memoirs the values of the electromotive forces in terms of the unit $D=1$. I also regard always as the point of issue, *i. e.* the positive part of the element in question, the amalgamated zinc in dilute sulphuric acid; so that, for example, the electromotive force of zinc in dilute sulphuric acid | Platinum in dilute sulphuric acid (or, abbreviated, $\text{Zn} | \text{Pt}$) $=1.61D$; Zinc in dilute sulphuric acid | Platinum coated with hydrogen in dilute sulphuric acid (or $\text{Zn} | \text{Pt}, \text{H}$) $=0.80D$, &c. Thus I found the force $\text{Zn} | \text{Pd}$, when I had only mechanically rubbed the oxidized plate, always very great, varying between 1.90 and 2.03 D , evidently because there were always some remains of oxide still adhering. But if the brown oxide was removed by diluted hydrochloric acid, the electromotive force was found to be constant within tolerably narrow limits—namely,

1.24, 1.26, 1.24, 1.29, 1.32, 1.31, 1.28.

Mean ... $\text{Zn} | \text{Pd}=1.28 D$.

If we may regard as actually pure the palladium thus cleaned, its position in the electromotive series is considerably nearer to zinc than that of platinum. Still it is not advisable, in measuring-experiments, to refer the position of a palladium plate polarized by any gas to that of pure palladium; it can be much more certainly ascertained if under all circumstances the amalgamated zinc plate in a concentrated solution of sulphate of zinc be united with the plate to be examined by the siphon to form a series, or if two plates polarized by different gases be placed immediately opposite one another.

Two strips cut from the same sheet of palladium were passed through corks which closed the upper ends of two glass tubes. The tubes were filled with diluted sulphuric acid and plunged upside down into a glass containing the same fluid. Oxygen was then introduced into one of the tubes, and hydrogen into the other. Both the gases had been evolved by electrolysis and kept in small gasometers, from which they could be taken as required.

The plate enveloped in oxygen exhibited not the slightest alteration in its electromotive quality, neither immediately nor after a longer-continued action of the oxygen. The difference of tension $\text{Zn} \mid \text{Pd}, \text{O}$ was invariably the same as $\text{Zn} \mid \text{Pd}$. The hydrogen gas, on the contrary, exerted a powerful influence from the first moment onwards: at the appearance of the first gas-bubble the palladium became at once positive; and after gas had been absorbed for some time by the metal the force $\text{Zn} \mid \text{Pd}, \text{H}$ was found, in different experiments made with plates or wires, to be

$$0.64, \quad 0.69, \quad 0.71, \quad 0.70, \quad 0.69.$$

$$\text{Mean} \dots \text{Zn} \mid \text{Pd}, \text{H} = 0.69 \text{ D.}$$

At this height it remained even when hydrogen had been introduced from without (or evolved on the plate itself) so long that the palladium could absorb no more, but free hydrogen enveloped its upper surface. According to this, the tension-difference would be

$$\text{Pd}, \text{H} \mid \text{Pd} = 1.28 - 0.69 = 0.59 \text{ D.},$$

while I had previously found

$$\text{Pt}, \text{H} \mid \text{Pt} = 0.81 \text{ D.}$$

Whether the palladium was employed bright, or coated with palladium-black, made no difference.

Further, palladium plates were used as the electrodes of a Grove's pile of three or four couples, or a Meidinger's pile of six couples. These electrodes were also enclosed in glass tubes, to enable me to continue the electrolysis until the hydrogen was no longer absorbed by the palladium. The measurement of the polarization present was, as before, effected by means of the universal compensator; with some practice the simple discharge obtained at it furnished very constant results, even though, like all similar contrivances, it was not entirely free from the inconvenience that the polarization-current was not closed till some, although a very short, time after the interruption of the polarizing current. To distinguish it from the electromotive force $\text{Zn} \mid \text{Pd}, \text{H}$, which was excited by merely enveloping a palladium plate in hydrogen, I denote by $\text{Zn} \mid \text{Pd}_\text{H}$ the force excited by the galvanic polarization of hydrogen. This was found to be

$$0.69, \quad 0.71, \quad 0.67,$$

$$\text{Mean} \dots \text{Zn} \mid \text{Pd}_\text{H} = 0.69 \text{ D.},$$

that is, exactly equal to $\text{Zn} \mid \text{Pd}, \text{H}$. In this case, therefore, no more hydrogen could be pressed into the palladium plate; the plate was already saturated with hydrogen.

Measurements executed with the positive electrode gave indefinite results. The plates immediately became brown and strongly negative, so that I obtained for the force $\text{Zn} \mid \text{Pd}_0$ values like 2.12 D. In correspondence with this, for the total polarization $\text{Pd}_H \mid \text{Pd}_0$ very great forces were found; but I convinced myself that their numerical evaluation was of no importance, since here not the action of gaseous active or passive oxygen comes into consideration, but that of the deposited layer of oxide. On this account, of the numerical data obtained by other observers on the strength of the polarization on palladium plates, I can only compare one with my own results: Graham*, namely, found that the polarization produced by from 1 to 4 Bunsen elements was

$$\text{Pd}_H \mid \text{Pd}_0 = 1.50 - 1.85 \text{ D.}$$

I find, on electrolysis by 4 Grove or 6 Meidinger,

$$1.83, \quad 1.77,$$

$$\text{Mean ... } \text{Pd}_H \mid \text{Pt}_0 = 1.80 \text{ D.},$$

therefore very nearly the same as Graham; but the platinum plate was not polarized to the maximum. A statement made by Pearnell†, according to which the polarization

$$\text{Pd}_H \mid \text{Pd}_0 = 0.306 \text{ D.},$$

is evidently much too low.

Covering palladium with palladium-black made no alteration in the polarization by hydrogen. Böttger‡ gives proofs of the powerful polarization of such blackened palladium plates; but the prominent action comes into consideration only when the closing of the current is continued, while with the momentary closing required by the compensation method it is of no importance. The palladium-black covering the positive electrode is immediately pushed off; the layer of oxide which forms completely exfoliates the black coating.

Of other gases, I have caused chlorine, carbonic oxide, ethylene, and sulphuretted hydrogen to act upon palladium.

The action of chlorine commences with the first traces that enter the fluid and are absorbed by it, and is strongly negative. When the fluid was saturated with chlorine there appeared the electromotive force

$$\text{Zn} \mid \text{Pd, Cl} = 2.04 \text{ D.};$$

on the combination standing longer, the force certainly con-

* Phil. Mag. [IV.] xxxviii. p. 243.

† Ibid. xxxix. p. 52.

‡ Jahresber. d. Frankf. ph. Ver. 1875-76, p. 23.

tinued to rise, but only slightly. Accordingly

$$\text{Pd} \mid \text{Pd, Cl} = 0.76 \text{ D.}$$

The attempt to polarize palladium with chlorine by electrolysis of hydrochloric acid had to be given up as useless. Even chlorine gas introduced from without into the gas element attacked the palladium and browned both the metal and the liquid after a time; while in the electrolysis the attack commenced immediately and with violence, and a coating of palladium-black was at once driven off.

Ethylene and carbonic oxide gas, into which the tube containing a palladium plate was introduced, polarized it positively: indeed, after introducing the ethylene, I found the values

$$1.22, \quad 1.24, \quad 1.23,$$

$$\text{Mean ... Zn} \mid \text{Pd, C}_2\text{H}_4 = 1.23 \text{ D.};$$

and after the introduction of the carbonic oxide,

$$1.05, \quad 1.06,$$

$$\text{Mean ... Zn} \mid \text{Pd, CO} = 1.05 \text{ D.}$$

Therefore

$$\text{Pd, C}_2\text{O}_4 \mid \text{Pd} = 0.05 \text{ D.}$$

$$\text{Pd, CO} \mid \text{Pd} = 0.23 \text{ D.}$$

When sulphuretted hydrogen was brought into the tube, I obtained immediately after the appearance of the first bubble the tension-difference

$$\text{Zn} \mid \text{Pd, H}_2\text{S} = 0.88 \text{ D.}$$

On continually agitating the liquid with fresh quantities of gas until it was saturated, the above difference was scarcely altered; after two successive fresh saturations I obtained

$$0.87 \text{ and } 0.87;$$

so that we have

$$\text{Pd, H}_2\text{S} \mid \text{Pd} = 0.41 \text{ D.}$$

The carbons with which I have experimented were four-edged rods of retort-carbon, such as are used for electric lamps. They are of great hardness and very close structure. They were purified by boiling in nitric acid, in water, and lastly in dilute sulphuric acid, in which they were then left to cool. If they were to be used in dilute hydrochloric instead of sulphuric acid, this liquid was then the last in which they were boiled. Through this treatment the rods were pretty homogeneous. Introducing them into dilute sulphuric acid, and uniting this by the siphon with the zinc-cell, I obtained the following elec-

tromotive forces :—

1·32	1·33	1·28	1·30	1·30	1·29
1·27	1·27	1·38	1·37	1·37	1·32

Mean ... Zn | C = 1·31 D.

For each series of experiments fresh carbons had to be employed, as the carbons, altered by the various actions which they had undergone, could not be again brought into their initial condition. Oxygen or hydrogen, led into the tubes enclosing the carbons, produced not the slightest result; the electromotive force of the combination remained quite unaltered = Zn | C. The behaviour of carbonic oxide and ethylene gas was just as indifferent. These results do not accord with my earlier experiences, according to which the gases mentioned acted as electromotors on Bunsen's carbon also, and by which I was induced to assume that the electromotive forces of carbon batteries composed of different metal (or carbon) plates, but of the same gases, stood in a definite relation, dependent on the condensing force exerted by the metals upon the gases. The carbons with which I worked thirty years since were very porous—battery carbons prepared from coke and coal; and at that time I said that the coefficient of condensation which I found for my carbons was certainly not to be regarded as holding good generally, but that other carbons might behave differently. Thus, with those now used such proportionality is altogether out of the question; the gases employed cannot have undergone any condensation upon the carbon. In order to test this rather unlikely fact more minutely, I cut out of such retort-carbon two regular-shaped pieces, each of 0·5 × 0·5 centim. cross section and 1 centim. length, the solid content of each being thus 0·25 cub. centim. These pieces were strongly heated and then introduced into ammonia gas which was enclosed in measuring-tubes over mercury. After the old temperature was fully restored, the volume of the ammonia gas had increased by a minute quantity which could not be precisely determined with the altered form of the meniscus. Had the increase amounted to 0·25 cub. centim., it would have been a proof that in fact no gas was absorbed; still the experiments showed that the retort-carbon had taken up as good as none of even this gas, which is briskly absorbed by the other sorts of carbon.

Very different was the behaviour of the carbon to chlorine. This gas was conducted into the tube of the element until it was no longer completely absorbed; then the connexion of the conducting liquid with the zinc-cell was restored, when the

following tension-differences were found:—

1·97, 1·97, 1·94, 2·01,

Mean ... $\text{Zn} \mid \text{C}, \text{Cl} = 0\cdot69 \text{ D};$

from which it follows that

$\text{C} \mid \text{C}, \text{Cl} = 0\cdot69 \text{ D}.$

When the chlorine was not brought into the tube from without, but evolved immediately at the carbon electrode by electrolysis of diluted hydrochloric acid, still greater electromotive forces were obtained—namely,

2·13, 2·25, 2·18,

Mean ... $\text{Zn} \mid \text{C}_{\text{Cl}} = 2\cdot19 \text{ D}.$

With longer-continued polarization Macaluso observed yet higher values.

That carbon electrodes are strongly polarized by electrolysis in dilute sulphuric acid is already known; Dufour*, particularly, has recently called attention to it. I found the polarization for both electrodes together

2·08, 2·21, 1·96, 2·04,

Mean ... $\text{C}_{\text{H}} \mid \text{C}_{\text{O}} = 2\cdot07 \text{ D}.$

After restoration of the connexion with the zinc-cell, there was found for the polarization of the negative electrode—

0·27, 0·26,

Mean ... $\text{Zn} \mid \text{C}_{\text{H}} = 0\cdot26 \text{ D};$

and for the positive—

2·16, 2·38,

Mean ... $\text{Zn} \mid \text{C}_{\text{O}} = 2\cdot27 \text{ D}.$

Further, by direct comparison were found:—the force between pure carbon and carbon polarized with hydrogen—

1·07, 1·11,

Mean ... $\text{C}_{\text{H}} \mid \text{C} = 1\cdot09 \text{ D};$

and between carbon pure and polarized with oxygen—

1·07, 1·04,

Mean ... $\text{C} \mid \text{C}_{\text{O}} = 1\cdot05 \text{ D}.$

Hence we should have

$\text{C}_{\text{H}} \mid \text{C}_{\text{O}} = 2\cdot14 \text{ D},$

while 2·07 had been found directly.

* *Bull. Soc. Vaud.* [2] xix. p. 63 (1876); *Wied. Ann. Beiblätter*, i. p. 573.

When the carbon electrodes at which the electrolysis had taken place were left in position, their difference of tension in comparison with pure carbon diminished only slowly and imperfectly. The carbon at which hydrogen had been evolved showed still, after twenty-four hours, tension-differences against pure carbon amounting to about 0.6 D, while that at which oxygen had been evolved showed about 0.3 D. Evidently, however, further chemical changes had taken place in the carbons:—in the negative, probably reductions of metallic oxides mixed with it in spite of all the purification it had undergone; in the positive, conversely, oxidations. An electrolysis, between carbon electrodes, of dilute sulphuric acid delivered, during the same time in which at platinum electrodes 27.36 cubic centims. of hydrogen were separated by the same current, 26.86 cubic centims. of hydrogen, but only 1.71 cubic centim. of oxygen. For the reduction, therefore, but very little hydrogen was consumed, and so much the more oxygen for the oxidation; indeed it was the carbon itself that was oxidized, forming carbonic acid and carbonic oxide gas. When, as in the present experiments, small quantities of gas are separated from large masses of conducting liquid, the carbonic acid is all absorbed; but if by long-continued electrolysis of a neutral-salt solution (for instance, Glauber salt) larger quantities of gas are evolved, the gas which collects above the liquid contains considerable amounts of free carbonic acid, which can be removed by agitation with caustic potash. The remaining gas proves to be carbonic oxide. The proportion of the two gases to one another appears to depend on the density of the current*. At the same time the carbon anode is violently attacked and carbon powder copiously thrown off from it, like the palladium dust thrown off from the oxidizing palladium plate; while the surface of the carbon becomes coloured deep blue. Macaluso has also observed this destruction of the carbon with the evolution of chlorine at a carbon electrode.

Different again, lastly, was the behaviour of carbon to sulphuric acid. After a few gas-bubbles had made their appearance at the carbon (just as before with palladium), no change was shown in its electromotive position. As the diluted sulphuric acid was repeatedly shaken with fresh quantities of

* In consequence of the complete disappearance of the gas in my first experiments, I at first thought that the carbon itself was not oxidized at all. An incidental communication from M. Laurent, Ingenieur, of Belfort, who had observed the occurrence of carbonic oxide and acid on electrolysis at carbon electrodes, induced me to repeat my experiments on a larger scale. I intend to carry them on still further.

sulphuretted hydrogen the carbon continually approached nearer to the positive end of the tension-series; there were observed, namely, for $\text{Zn} \mid \text{C}, \text{H}_2\text{S}$:—

At the commencement . . .	1.29
After the second saturation . .	1.13
After the third ,, . .	1.04
After the fourth ,, . .	1.02

Thus, with the saturation of the solution, the electromotive force approached towards a limiting value which is to be set down as about



so that



The electromotive forces which were called forth at the palladium by hydrogen, sulphuretted hydrogen, carbonic oxide, and ethylene show, in fact, again a similar proportionality, as I had previously conjectured for all the metals. In the following Table I place side by side the values before found for platinum and those now found for palladium, and calculate from the forces observed at platinum those to be expected at palladium, by multiplying the former with the ratio



	Pt. Found.	Pd. Found.	Pd. Calculated.
H . . .	0.81	0.59	0.59
H_2S . .	0.69	0.42	0.50
CO . . .	0.28	0.23	0.20
N_2H_4 . .	0.06	0.05	0.04
Metal . .	0	0	0

For the retort-carbon, on the contrary, nothing similar is to be observed; its state was in general changed *only* under the influence of greater solubility of the gases or the electric polarization. Besides we have no longer any right to designate the factor 0.73 as the coefficient of condensation for palladium, since we know that palladium condenses hydrogen much more strongly than platinum does.

From the results obtained the following is now evident:—Platinum, palladium, and carbon behave to chlorine exactly alike, so much so that the numerical values found for the electromotive forces $\text{Zn} \mid \text{Pt}, \text{Cl}$; $\text{Zn} \mid \text{Pd}, \text{Cl}$; $\text{Zn} \mid \text{C}, \text{Cl}$ stand very near to one another; they amount to 2.08, 2.04, 1.97 D respectively. The values which were obtained on the electrolytic evolution of chlorine are here left out of consideration, because the attack which therein took place on the elec-

trodes makes the comparison unsafe. The almost perfect agreement between Pt, Cl and C, Cl was also remarked by Macaluso*. It looks as if the plate dipping in the chlorine solution served solely as a conductor; and in fact we cannot here speak of the electromotive force excited by a gas, but have simply to do with the electromotive action of a liquid, which increases with the degree of concentration of the liquid.

The solubility of sulphuretted hydrogen in water is similar to that of chlorine; but it behaves otherwise to platinum and palladium than to retort-carbon. The latter, again, occurs only as a body immersed in a solution, by which it is the more intensely electrically excited the more concentrated the solution. Platinum and palladium are already strongly excited by the first quantities of gas; they evidently draw it from the liquid to condense it in or upon themselves.

The rest of the gases which have been taken into consideration are but little soluble in water. Of course, in the usual form of the gas battery, something even of these must at first be dissolved in the conducting liquid in order to become active; but the quantity is too inconsiderable to cause the solution to act on the conducting plate essentially otherwise than the liquid which has absorbed no gas at all. In these cases something else must come into play to generate a difference of tension—namely, either an affinity (or, generally, an action of molecular forces by which the gases incorporate themselves with the metal plate), or the action of an electrolyzing current which either drives the gases into the metal or condenses them upon its surface. On palladium hydrogen exhibits this penetration in the highest degree, on platinum in a less degree, on retort-carbon not at all. The aid of galvanic polarization is superfluous with palladium, useful with platinum, absolutely necessary with retort-carbon to generate a difference of tension. Carbonic oxide and ethylene act in the same manner as hydrogen, but far more feebly. If we could condense them by galvanic polarization, it would in all three cases be useful; with carbon, indeed, it would be indispensable. Sulphuretted hydrogen stands, with reference to its behaviour to platinum and palladium, near to hydrogen, and near chlorine in consequence of its solubility.

I made an experiment to ascertain if chlorine, which so readily attacks the surface of metals, penetrates also into or through them. Exactly as in Root's experiment, two glass vessels were cemented to the two sides of a much broader sheet of palladium *b*. Both vessels were filled with diluted hydrochloric acid; and palladium electrodes *a* and *c* dipped

* *Loc. cit.* p. 362.

into them. Between *a* and *b* a current was closed, so that chlorine was evolved upon the side of *b* turned towards *a*. On the other hand, *b* and *c* could be connected with the galvanometer by momentary closings. To my astonishment, after a time there was shown an electric difference in which, not *b*, but *c* appeared negative. Of the liberated chlorine, traces passed through the atmosphere to the surface of the liquid in the other vessel, and through it arrived at the electrode *c*. That slight traces of chlorine act at once electromotively on platinum also had already been remarked by Macaluso; and I am now of opinion that the oxygen gas which I used for my first measurements on gas batteries, and which had been prepared from chlorate of potass, always carried with it traces of chlorine, although I thought I had sufficiently purified it by washing; for with oxygen obtained by electrolysis I could as little excite platinum electromotively as palladium. I now altered my apparatus by giving it the form of a U-shaped tube, the horizontal part of which, 80 centims. long, was divided in the middle by a thin plate of palladium into two halves. I first filled both sides with diluted sulphuric acid, and evolved hydrogen at the side of *b* facing the plate *a*, and that by closing the circuit for only a few seconds. The action of hydrogen that had penetrated through the palladium was very soon perceptible; the plate *b* also became positive on its reverse side. The experiment cannot be long continued; for the plate bends so much that it soon breaks loose from its attachment. A fresh tube was now filled with diluted hydrochloric acid. The long layer of liquid permitted none of the evolved chlorine to escape, while the electrode *c* remained perfectly indifferent until the plate *b* was eaten quite through. In order to fix more exactly the instant at which this took place, I filled the vertical parts of the U-tube up to as many different heights as possible with the liquid, and repeated the experiment. Again *b* and *c* remained indifferent to one another; suddenly there was a violent deflection of the galvanometer-mirror; but at this moment the liquid on both sides began to place itself in equilibrium. According to these experiments, chlorine does not penetrate palladium as hydrogen does.

From this I think I am warranted in maintaining, generally, that, strictly speaking, we never have to do with any electromotive force of gases, but either with tension-differences called forth by conducting liquids of different kinds, or with alterations of metals by gases which have lost their gaseous state by occlusion in, or condensation on the surface of, metals; for an actually coherent layer of gas that covered a

metallic conductor would surely insulate it from the conducting liquid.

I will here add the description of an experiment which I made, long ago, for the purpose of getting an explanation of the activity of gases in the gas battery. Gaugain, in the paper above mentioned, has advocated the view that the electromotive force of the gas battery is to be attributed solely to the chemical affinity with which the oxygen of the water and the hydrogen condensed by the platinum act on one another. To this I objected that this proposition must be generalized, since other gases also act as electromotors; it must therefore be expressed something like this:—A gas acts as an electromotor through combining, under a catalytic cooperation of the platinum, with one of the elements of the water*. Whether this proposition is correct can be ascertained by the following experiment. In a dark room I filled up two tubes, in each of which was a platinum plate, and containing, as usual, diluted sulphuric acid, with chlorine. The two plates showed no difference of tension. I now covered over one of the tubes with a yellow-glass bell, and let the daylight fall upon both tubes. Certainly the action of the chlorine upon the hydrogen of the water was now much more vigorous in the free than in the covered tube; but no difference of tension was visible. For chlorine, therefore, the above proposition is certainly untenable. To hydrogen it is indeed still less applicable, since otherwise the affinity of the hydrogen on the platinum for the oxygen of the water would have to be greater than that of the oxygen for the hydrogen already combined with it.

I remark finally, in reference to Graham's statement (already called in question by G. Wiedemann†) that palladium charged with hydrogen is strongly magnetic, that I have never succeeded in detecting any action of hydride of palladium upon the magnetometer.

After the above communication had been presented to the Royal Academy, I received the April number of the 'Philosophical Magazine,' in which Mr. Morley publishes an investigation, carried out by him in Professor Foster's laboratory, on Grove's gas battery. Mr. Morley is only acquainted with the older writings of Grove and Schönbein and the newer ones of Gaugain; mine he seems never to have seen.

* Pogg. *Ann.* cxxxii. p. 458.

† *Galvanismus*, 2nd. ed., vol. i. p. 528; cf. Blondlot, *Beibl.* vol. i. p. 634.

He, likewise, controverts the view that the seat of the electromotive force in gas batteries is the place of contact of metal, liquid, and gas; but he comes to the conclusion which in the present communication I have declared is not universally valid—that the entire current of the gas battery owes its rise to the dissolved gases. At the same time he does not admit that the gradual falling-off of the current of a closed gas battery is to be attributed to polarization coming in, but seeks its cause solely in the diminution of the volume of gas dissolved in the liquid. As, however, he does not measure the electromotive forces by momentary closings of the circuit, as Gaugain and I have done, but calculates them from the current-intensity observed during a continued closing, and from the resistance, it is not possible from his measurements to distinguish the primary from the secondary actions. That a mixture of this sort has not been avoided is shown also by the proposition at which Mr. Morley arrives:—that the electromotive force of the gas battery is not constant, but increases with the resistance.

Munich, May 1878.

II. *On the Mechanical Theory of Crookes's Force.*

By GEORGE FRANCIS FITZGERALD, M.A., F.T.C.D.*

WHEN two surfaces at different temperatures are in presence of one another with a gas between them, there exists a force tending to separate them. The assumption of this force explains a very great number of phenomena, including the motion of the arms in Mr. Crookes's radiometers, and the so-called spheroidal state of liquids. That this force was due to some sort of unequal stress in the gas between the two surfaces, was pointed out by Mr. Stoney in the *Philosophical Magazine*, March and April 1876, where he attempted to show that such a state of stress would arise. An attempt to explain the motion of the arms of a radiometer had been made previously by Professor O. Reynolds; but his conclusion, that it was principally due to evaporation and condensation, is manifestly inadequate to explain a continuous action, such as that in a radiometer; and the method by which he tried to show that a surface, when communicating heat to gas, is subject to an increased pressure, is open to the overwhelming objection that this increased pressure would be almost instantaneously transmitted to all parts of the enclosed gas, and so

* From the *Scientific Transactions of the Royal Dublin Society* for October 1878. Communicated by the Author.

could not possibly be the source of such a force as would explain the motion of the arms of a radiometer.

In amplification of a letter I wrote to 'Nature' on the 17th of December 1877, and which was published on the 4th of January, 1878, I now intend to prove that such a state of stress as Mr. Stoney's theory requires would exist under the assumed conditions. My letter contains a proposed application of Clausius' investigation for finding the conducting-power of a gas, as published in the *Philosophical Magazine*, vol. xxiii. 4th series. Mr. Stoney, in a paper read before the Royal Dublin Society on Monday, the 18th of February, 1878, [*Phil. Mag.* Dec. 1878, p. 401] has obtained results somewhat like those obtained by my method by applying a method similar to one he originally employed.

I may first observe that the only way in which a state of other than uniform stress can exist in a gas is by the distribution of the mean velocities, and number of molecules, being different in different directions, or, as Mr. Stoney has called it, by the gas being polarized. That the distribution is not uniform when heat is being conducted through a gas has been pointed out long ago by both Clausius and Maxwell; and what is required is, to show that the distribution will then be such as to develop a force like Crookes's.

Following the method adopted by Clausius in his paper already referred to, I assume that the mean velocity of a molecule is a function of its direction of motion, and that the number of molecules in the unit volume moving in a given direction is also a function of that direction. If, then, we define the direction by means of μ , the cosine of the angle it makes with a given direction, ϕ the angle the plane of these two directions makes with a fixed plane through the given direction, we may evidently assume

$$v = v_0 f(\mu\phi), \quad n = n_0 F(\mu\phi),$$

where v and n are the mean velocities and number of molecules moving in this direction, and v_0 and n_0 are certain given values of v and n when f and F are unity. Now we may evidently in addition take $n_0 = \frac{N}{4\pi} N d\mu d\phi$, where N is the total number of molecules per unit of volume; so that we have, generally,

$$n = \frac{N}{4\pi} F(\mu, \phi) d\mu d\phi.$$

The quantities I intend to calculate are—the number of molecules carried through the unit area in any direction, the total

momentum carried through the same, and the quantity of energy carried through it. The number of molecules going in one direction through the unit area must evidently be equal to that of those going in the opposite direction, if there are no gaseous currents going on; and even if present, their existence is evidently beside the question in hand. Hence, if we sum the number of molecules passing the unit area, taking those that go in opposite direction through it with opposite signs, the sum must vanish. I shall calculate the numbers in three cases of unit areas:—1st, perpendicular to the line from which π is measured, or X; 2nd, parallel to the plane from which ϕ is measured (*i. e.* perpendicular to Y); and, 3rd, for the case of a unit area perpendicular to these two (*i. e.* perpendicular to Z). The number of molecules going in the direction (μ, ϕ) that pass through the first of these per unit of time is evidently $=nv\mu$; and it is likewise evident that the number going in the opposite direction will have an opposite sign; so that we have the sum of all such zero. Similarly, for the other two planes the numbers are

$$nv\sqrt{1-\mu^2} \sin \phi \text{ and } nv\sqrt{1-\mu^2} \cos \phi;$$

so that we get

$$0 = \Sigma nv\mu = \Sigma nv\sqrt{1-\mu^2} \sin \phi = \Sigma nv\sqrt{1-\mu^2} \cos \phi.$$

The momentum carried through the first of these unit areas per unit of time by molecules moving in the direction (μ, ϕ) is $=Mnv^2\mu^2$, if M be the mass of each molecule; and as it does not change sign with μ , we see that the sum of all such will represent the normal pressure per unit area at the given place. We can similarly get the normal pressures on the other two unit areas; and calling them P_{xx} , P_{yy} , and P_{zz} , we obtain

$$P_{xx} = M\Sigma nv^2\mu^2,$$

$$P_{yy} = M\Sigma nv^2(1-\mu^2) \sin^2 \phi,$$

$$P_{zz} = M\Sigma nv^2(1-\mu^2) \cos^2 \phi.$$

Proceeding similarly, we can get the tangential pressures on these areas; and we easily see that they are

$$P_{yz} = P_{zy} = M\Sigma nv^2(1-\mu^2) \sin \phi \cos \phi,$$

$$P_{zx} = P_{xz} = M\Sigma nv^2\mu\sqrt{1-\mu^2} \cos \phi,$$

$$P_{xy} = P_{yx} = M\Sigma nv^2\mu\sqrt{1-\mu^2} \sin \phi.$$

If now we proceed to calculate the energy carried across these areas per unit of time, we get $knv^3\mu$ as that carried across the first area by molecules moving in the direction (μ, ϕ) when k is the coefficient by which the energy of translation must

be multiplied in order to obtain the total energy. Calling the quantities of energy Q_x , Q_y , Q_z , we thus get

$$Q_x = Mk \Sigma n v^3 \mu,$$

$$Q_y = Mk \Sigma n v^3 \sqrt{1-\mu^2} \sin \phi,$$

$$Q_z = Mk \Sigma n v^3 \sqrt{1-\mu^2} \cos \phi.$$

In order to be able to perform these summations, it is necessary to know the mean values of nv , nv^2 , and nv^3 in terms of μ and ϕ ; and I shall, in the first place, merely assume that they can be expanded in a series of spherical harmonics, thus:

$$\overline{nv} = \frac{N v_0}{4\pi} (A_0 + A_1 + A_2 + \dots) d\mu d\phi,$$

$$\overline{nv^2} = \frac{N v_0^2}{4\pi} (B_0 + B_1 + B_2 + \dots) d\mu d\phi,$$

$$\overline{nv^3} = \frac{N v_0^3}{4\pi} (C_0 + C_1 + C_2 + \dots) d\mu d\phi.$$

The effect of this is to obtain our former results under the following simplified forms. Our first series of equations gives $A_1 = 0$; and as A_1 must be of the form

$$A_1 = a_1 \mu + a_2 \sqrt{1-\mu^2} \sin \phi + a_3 \sqrt{1-\mu^2} \cos \phi,$$

we get

$$a_1 = a_2 = a_3 = 0.$$

The second system of equations gives

$$P_{xx} = \frac{MN v_0^2}{4\pi} \iint (B_0 + B_2) \mu^2 d\mu d\phi,$$

$$P_{yy} = \frac{MN v_0^2}{4\pi} \iint (B_0 + B_2) (1-\mu^2) \sin^2 \phi d\mu d\phi,$$

$$P_{zz} = \frac{MN v_0^2}{4\pi} \iint (B_0 + B_2) (1-\mu^2) \cos^2 \phi d\mu d\phi,$$

$$P_{yz} = P_{zy} = \frac{MN v_0^2}{4\pi} \iint B_2 (1-\mu^2) \sin \phi \cos \phi d\mu d\phi,$$

$$P_{zx} = P_{xz} = \frac{MN v_0^2}{4\pi} \iint B_2 \mu \sqrt{1-\mu^2} \cos \phi d\mu d\phi,$$

$$P_{xy} = P_{yx} = \frac{MN v_0^2}{4\pi} \iint B_2 \mu \sqrt{1-\mu^2} \sin \phi d\mu d\phi.$$

If now we assume

$$B_2 = b_1(\mu^2 - \frac{1}{3}) + b_2(1 - \mu^2) \cos 2\phi + b_3(1 - \mu^2) \sin \phi \cos \phi \\ + b_4\mu\sqrt{1 - \mu^2} \cos \phi + b_5\mu\sqrt{1 - \mu^2} \sin \phi,$$

as it must be of this form, we get, on putting our other quantities into the forms of spherical harmonics,

$$P_{xx} = \frac{MNv_0^2}{3} \left(B_0 + \frac{4}{15} b_1 \right),$$

$$P_{yy} = \frac{1}{3} MNv_0^2 \left(B_0 - \frac{2}{15} b_1 - \frac{2}{5} b_2 \right),$$

$$P_{zz} = \frac{1}{3} MNv_0^2 \left(B_0 - \frac{2}{15} b_1 + \frac{2}{5} b_2 \right),$$

$$P_{yz} = \frac{1}{15} MNv_0^2 b_3 = P_{zy},$$

$$P_{zx} = \frac{1}{15} MNv_0^2 b_4 = P_{xz},$$

$$P_{xy} = \frac{1}{15} MNv_0^2 b_5 = P_{yx}.$$

Similarly, for the quantities of energy transferred we get

$$Q_x = \frac{MNv_0^3}{4\pi} k \iint C_1 \mu d\mu d\phi,$$

$$Q_y = \frac{MNv_0^3}{4\pi} k \iint C_1 \sqrt{1 - \mu^2} \sin \phi d\mu d\phi,$$

$$Q_z = \frac{MNv_0^3}{4\pi} k \iint C \sqrt{1 - \mu^2} \cos \phi d\mu d\phi;$$

so that if we assume, as we evidently may,

$$C_1 = c_1\mu + c_2\sqrt{1 - \mu^2} \sin \phi + c_3\sqrt{1 - \mu^2} \cos \phi,$$

we get

$$Q_x = \frac{k}{3} MNv_0^3 c_1,$$

$$Q_y = \frac{k}{3} MNv_0^3 c_2,$$

$$Q_z = \frac{k}{3} MNv_0^3 c_3.$$

Even in this most general form we can see that there will in general be a difference of pressure in different directions;

for it is evident that the pressures in the three directions cannot be equal unless b_1 and b_2 both vanish, which will not in general be the case. Without a knowledge of the nature of the distribution of the velocities and numbers of molecules moving in the different directions, it would be impossible to calculate the values of b_1 , b_2 , b_3 , b_4 , and b_5 ; but I think we can see that they will in part at least vary as the square of the quantity of heat passing. This can be seen from the following considerations. No matter what the distribution of the velocities and numbers of molecules moving in the different directions may be, it is plain that terms occurring in the coefficients of $\sqrt{1-\mu^2} \sin \phi \sqrt{1-\mu^2} \cos \phi$ (*i. e.* in the spherical harmonics of the first order in u and v) will occur in the terms of the same order in nv , nv^2 and nv^3 , and that linearly; while these same terms will occur squared in the spherical harmonics of the second order in nv , nv^2 and nv^3 . Hence we see that terms occurring linearly in the spherical harmonics of the first order in nv^3 will occur as squares in the spherical harmonics of the second order in nv^2 ; so that b_1 , b_2 will contain c_1 , c_2 , and c_3 in the second degree, *i. e.* will contain terms varying as the squares of the quantities of heat passing. It is also to be observed that terms occurring in the spherical harmonics of the second order can never come into those of the first, except as products with terms belonging to spherical harmonics of the third order; so that a hypothetical distribution which gave correct values for the quantities of heat passing might very well be quite inadequate as a means of calculating the difference of pressure in different directions. This remark is of importance when we come to consider the results of Clausius' hypothesis, and was suggested to me by Mr. Stoney in conversation.

As an example of what I am insisting upon, we may take two opposite extreme cases:—first, the case of B_2 vanishing, and, secondly, the case of C_1 doing so. In the first case there would be a distribution of velocities and numbers such that, though heat would be conducted across the layer, nevertheless there would be no resultant inequality of stress; while in the second case, though no heat would be conducted, yet there would be inequality of stresses. It seems, however, certain that neither of these extreme cases can exist as a permanent distribution in gases. Before calculating the values of these quantities upon particular hypothetical distributions, it may be well to see what they are in the simple case of two parallel planes, each at a uniform temperature.

In this case it is evident from symmetry that, if we take X normal to the planes, we must have all our equations indepen-

dent of ϕ , as the effect is evidently symmetrical with regard to X. Then we get

$$b_2 = b_3 = b_4 = b_5 = 0 = c_2 = c_3,$$

and there are no tangential forces, while all the heat is transferred in the direction X, and our pressures become

$$P_{xx} = \frac{1}{3} MN v_0^2 \left(B_0 + \frac{4}{15} b_1 \right),$$

$$P_{yy} = P_{zz} = \frac{1}{3} MN v_0^2 \left(B_0 - \frac{2}{15} b_1 \right),$$

while the heat transferred is

$$Q_z = \frac{k}{3} MN v_0^3 c_1.$$

The excess of pressure in X over that in the normal directions is

$$P_{xx} - P_{yy} = \frac{2}{15} MN v_0^2 b_1 = K;$$

and this has been called Crookes's force.

That it depends wholly upon b_1 can be seen by the following simple method, mentioned to me by Mr. Stoney.

Our expressions for P_{xx} and P_{yy} are

$$P_{xx} = M \Sigma n v^2 \mu^2,$$

$$P_{yy} = M \Sigma n v^2 (1 - \mu^2) \sin^2 \phi;$$

so that, calling

$$n = \frac{N}{4\pi} I d\mu d\phi,$$

when I depends upon the distribution of numbers only, we can write the pressures

$$P_{xx} = \frac{MN}{4\pi} \iint I v^2 \mu^2 d\mu d\phi,$$

$$P_{yy} = \frac{MN}{4\pi} \iint I v^2 (1 - \mu^2) \sin^2 \phi d\mu d\phi.$$

We can integrate them with respect to ϕ ; for we know that Iv^2 is independent of ϕ in the case we are considering;

$$\therefore P_{xx} = \frac{1}{2} MN \int I v^2 \mu^2 d\mu,$$

$$P_{yy} = \frac{1}{4} MN \int I v^2 (1 - \mu^2) d\mu;$$

$$\therefore P_{xx} - P_{yy} = \kappa = \frac{3}{4} MN \int I v^2 (\mu^2 - \frac{1}{3}) d\mu;$$

so that if Iv^2 be expanded in spherical harmonics, K depends only upon the spherical harmonic of the second order. Similarly, if Iv^3 be similarly expanded, it is easy to see that

$$Q_x = \frac{1}{2} MNk \int Iv^3 \mu d\mu$$

can only depend upon the spherical harmonic of the first order in Iv^3 .

If now we turn to particular hypotheses as to the character of the distribution of velocities and numbers, the first that claims our attention is Clausius's. He starts from the assumption that the distribution of velocities among the molecules that have just encountered one another in any given layer may be perfectly represented by supposing a small constant velocity in the direction of the transference of heat to be superposed upon a uniform distribution. This is the same as supposing that these velocities in various directions may be represented by the radii drawn to the surface of a sphere from a point slightly displaced from its centre. It is worthy of remark, in connexion with what I mentioned before with reference to the way the quantities in the various spherical harmonics are related to one another, that, supposing the sphere to be an ellipsoid of even greater ellipticity would not have affected his results; for it is easy to show that the ellipticity of an ellipsoid of revolution only enters into the spherical harmonics of the second and higher orders; so that it would not enter into the equation giving the quantity of heat, except when multiplied by terms of at least the order of the quantity of heat. Thus, even though the square of the ellipticity were of the order of the displacement from the centre of the point from which the radii representing the velocities are drawn, nevertheless that would at most only have introduced terms depending upon the product of these two, which would not have materially affected his results. Hence we see that Clausius' success in calculating the quantity of heat conducted is no proof that his hypothesis is by any means a sufficient representation of the actual distribution for the purpose of calculating the resultant stresses; and that it is not proved by calculating what the Crookes's force would be upon his hypothesis. If this be done with the help of the quantities he gives in his note (see *Phil. Mag.* [IV.] vol. xxiii. p. 526), we get

$$K = \frac{1 \cdot 8}{k^2} \cdot \frac{\rho_0 T_0}{P_0} \cdot \frac{Q^2}{PT};$$

and the pressures deduced from this formula are very much smaller than those observed; so that it seems certain that the

hypothetical distribution Clausius assumed is not at all adequate to represent the actual one. The pressures obtained by this formula are so insignificant that it is not worth while giving the details of the method by which it is deduced. That Clausius' hypothesis is by no means adequate, can also be seen by the consideration that it is only after the Clausian laws for the conduction of heat have ceased to apply, owing to the rarefaction of the gas, that Crookes's force becomes remarkable, as well as by considering what the distribution tends towards, as has been done by Mr. Stoney, in his paper published in the December Number of this Magazine. He shows that the distribution lies between one which could be represented by two unopposing streams of molecules, moving one towards the heater and the other towards the cooler and unpolarized gas. With such a distribution the laws of conduction of heat would, of course, differ somewhat from those deduced from Clausius' distribution.

I shall now calculate the result upon an arbitrarily assumed distribution, which, however, probably represents the actual one more nearly than Clausius's. I shall assume that the distribution of velocities can be represented by the formula

$$v = v_0(1 + \alpha \cos \theta + \beta \sin \theta \sin \phi + \gamma \sin \theta \cos \phi \\ + a \cos \theta + b \sin^2 \theta \sin^2 \phi + c \sin^2 \theta \cos^2 \phi + 2f \sin^2 \theta \sin \phi \cos \phi \\ + 2g \sin \theta \cos \theta \cos \phi + 2h \sin \theta \cos \theta \sin \phi,$$

where

$$\cos \theta = \mu.$$

This is equivalent to saying that it is represented very nearly by the radii drawn to the surface of a slightly elliptical ellipsoid from a point near its centre. I shall assume that $\alpha, \beta, \gamma, a, b, c, f, g, h$ are all quantities whose squares and products may be neglected. For the number of molecules moving in the given direction θ, ϕ , I shall assume that it varies inversely as the velocity of the molecules moving in that direction, so that $nv = Nv_0$. This evidently satisfies the condition $A_1 = 0$. By these assumptions we obtain approximately $nv^2 = Nv_0 \cdot v$ and $nv^3 = Nv_0 \cdot v^2$, and hence

$$nv^2 = Nv_0^2 \left\{ \begin{aligned} &[1 + \alpha\mu + \beta\sqrt{1-\mu^2} \sin \phi + \gamma\sqrt{1-\mu^2} \cos \phi \\ &+ \alpha\mu^2 + b(1-\mu^2) \sin^2 \phi + c(1-\mu^2) \cos^2 \phi \\ &+ 2f\sqrt{1-\mu^2} \sin \phi \cos \phi + 2g\mu\sqrt{1-\mu^2} \cos \phi \\ &+ 2h\mu\sqrt{1-\mu^2} \sin \phi], \end{aligned} \right\}$$

or, turning it into the form of a series of spherical harmonics,

$$nv^2 = Nv_0^2 \left\{ \begin{aligned} &1 + \frac{1}{3}(a+b+c) + \left(a - \frac{1}{2}\overline{b+c}\right)(\mu^2 - \frac{1}{3}) \\ &\quad + \frac{1}{2}(c-b)(1-\mu^2)\cos 2\phi \\ &+ 2f\sqrt{1-\mu^2} \cdot \sin \phi \cos \phi + 2g\mu\sqrt{1-\mu^2} \cdot \cos \phi \\ &\quad + 2f\mu\sqrt{1-\mu^2} \cdot \sin \phi \\ &+ \alpha\mu + \beta\sqrt{1-\mu^2} \cdot \sin \phi + \gamma\sqrt{1-\mu^2} \cdot \cos \phi, \end{aligned} \right\}$$

from which we see that

$$b_1 = a - \frac{1}{2}(b+c), \quad b_2 = \frac{1}{2}(c-b),$$

$$b_3 = 2f, \quad b_4 = 2g, \quad b_5 = 2h.$$

We may evidently include the $\frac{1}{3}(a+b+c)$ in the mean value of Nv_0^2 , and take $B_0 = 1$; so that, calling $MN = \rho$ the density of the gas, our pressures become

$$P_{xx} = \frac{1}{3} \rho v_0^2 \left[1 + \frac{4}{15} \left(a - \frac{1}{2}(b+c) \right) \right],$$

$$P_{yy} = \frac{1}{3} \rho v_0^2 \left[1 + \frac{4}{15} \left(b - \frac{1}{2}(c+a) \right) \right],$$

$$P_{zz} = \frac{1}{3} \rho v_0^2 \left[1 + \frac{4}{15} \left(c - \frac{1}{2}(a+b) \right) \right],$$

$$P_{yz} = \frac{2}{15} \rho v_0^2 \cdot f = P_{zy},$$

$$P_{zx} = \frac{2}{15} \rho v_0^2 \cdot g = P_{xz},$$

$$P_{xy} = \frac{2}{15} \rho v_0^2 \cdot h = P_{yx}.$$

Similarly, from $nv^3 = Nv_0 \cdot v^2$ we can get

$$c = 2\alpha, \quad c^2 = 2\beta, \quad c^3 = 2\gamma,$$

and hence

$$Q_x = \frac{2}{3} \overline{k\rho v_0^3} \cdot \alpha, \quad Q_y = \frac{2}{3} \cdot \overline{k\rho v_0^3} \cdot \beta, \quad Q_z = \frac{2}{3} \overline{k\rho v_0^3} \cdot \gamma.$$

The normal pressures may also be put into the form

$$P_{xx} = \frac{1}{3} \rho v_0^2 \cdot \left\{ 1 + \frac{1}{15} (a+b+c) + \frac{1}{5} (a-b-c) \right\},$$

$$P_{yy} = \frac{1}{3} \rho v_0^2 \cdot \left\{ 1 + \frac{1}{15} (a+b+c) + \frac{1}{5} (b-c-a) \right\},$$

$$P_{zz} = \frac{1}{3} \rho v_0^2 \cdot \left\{ 1 + \frac{1}{15} (a+b+c) + \frac{1}{5} (c-a-b) \right\};$$

so that the state of stress is a uniform pressure, and superposed upon it a system of pressures represented by the equations

$$p_{xx} = \frac{1}{3} \rho v_0^2 \cdot \frac{1}{5} (a-b-c),$$

$$p_{yy} = \frac{1}{3} \rho v_0^2 \cdot \frac{1}{5} (b-c-a),$$

$$p_{zz} = \frac{1}{3} \rho v_0^2 \cdot \frac{1}{5} (c-a-b),$$

$$p_{yz} = \frac{1}{3} \rho v_0^2 \cdot \frac{2}{5} f = p_{zy},$$

$$p_{zx} = \frac{1}{3} \rho v_0^2 \cdot \frac{2}{5} g = p_{xz},$$

$$p_{xy} = \frac{1}{3} \rho v_0^2 \cdot \frac{2}{5} h = p_{yx}.$$

Now it is remarkable that, if

$$ax^2 + by^2 + cz + 2yz + 2gzx + 2hxy = (lx + my + nz)^2,$$

we should have expressions for these additional unequal pressures the same as Professor Clerk Maxwell gives (see his 'Electricity and Magnetism,' vol. i. p. 129, and vol. ii. p. 256) as expressing that state of stress in the æther which produces electrical phenomena. In order to make them identical, all that is necessary is to put

$$X = l \sqrt{\frac{8\pi}{15} \rho v_0^2},$$

$$Y = m \sqrt{\frac{8\pi}{15} \rho v_0^2},$$

$$Z = n \sqrt{\frac{8\pi}{15} \rho v_0^2};$$

so that the resultant unequal pressures in the gas may be

represented by a pressure $p = \frac{R^2}{8\pi}$ when $R^2 = X^2 + Y^2 + Z^2$ in the direction given by

$$\mu : \sqrt{1-\mu^2} \sin \phi : \sqrt{1-\mu^2} \cos \phi :: X : Y : Z :: l : m : n,$$

and an equal diminished pressure in every direction at right angles to this line. Double this pressure will be the Crookes's force, which is consequently in this case

$$K = \frac{1}{3} \rho v_0^2 \cdot \frac{1}{5} (l^2 + m^2 + n^2);$$

and it is in the direction whose direction-cosines are proportional to $l : m : n$; so that, if we put

$$l = v\mu, \quad m = v\sqrt{1-\mu'^2} \sin \phi, \quad n = v\sqrt{1-\mu'^2} \cos \phi,$$

$$R = \frac{1}{15} \rho v_0^2 \cdot v^2.$$

The direction-cosines of the line of transference of heat are evidently $\alpha : \beta : \gamma$, and so far there is no reason why these two lines should coincide, although of course in most cases they probably differ but little in direction.

The only other distribution I shall consider is one suggested by Mr. Stoney's investigation (*Scientific Transactions of the Royal Dublin Society*, p. 39) of the nature of the distribution of the velocities in the gas between two large parallel surfaces at uniform unequal temperatures. He has shown that it tends towards a distribution which would be represented by two streams of unpolarized gas moving in opposite directions across the layer. Now the actual distribution is never exactly this, and possibly, as he has mentioned, departs in various degrees from it as you pass across the layer. If, however, we assume the distribution to be the same all the way across, and that consequently the mean temperature of each stream is that due to the surface it is leaving, we can calculate the resultant pressures.

If v_1 and v_2 be the mean velocities of the molecules in each stream respectively relatively to the centres of mass of the molecules, and if u_1 and u_2 be the velocities of the streams (*i. e.* of these centres of mass), and ρ_1 and ρ_2 their densities, the pressure upon a fixed plane normal to the direction of the streams is

$$P = \frac{1}{3} \rho_1 v_1^2 + \frac{1}{3} \rho_2 v_2^2 + \rho_1 u_1^2 + \rho_2 u_2^2,$$

while the pressure sideways is

$$p = \frac{1}{3} \rho_1 v_1^2 + \frac{1}{3} \rho_2 v_2^2;$$

so that the Crookes's pressure in this case is

$$K = P - p = \rho_1 u_1^2 + \rho_2 u_2^2.$$

In order that there be no accumulation of gas at either surface, we must evidently have

$$\rho_1 u_1 = \rho_2 u_2.$$

If V_1^2 and V_2^2 be the total mean squares of the velocities of agitation, $V_1^2 = v_1^2 + u_1^2$, $V_2^2 = v_2^2 + u_2^2$, and the quantity of heat transferred is

$$Q = k(\rho_1 V_1^2 u_1 - \rho_2 V_2^2 u_2),$$

k being, as before, the coefficient by which the *vis viva* of translation has to be multiplied in order to get the total energy of the gas.

From these we easily obtain

$$K = \rho_1 u_1 (u_1 + u_2),$$

$$Q = k \rho_1 u_1 (V_1^2 - V_2^2);$$

$$\therefore Q = kK \cdot \frac{V_1^2 - V_2^2}{u_1 + u_2}.$$

We have besides $\rho_1 + \rho_2 = \rho$, where ρ is the density of the gas. Hence there are six equations between the six unknowns,

$$\rho_1, \rho_2, v_1, v_2, u_1, u_2;$$

and in order to eliminate them and obtain an equation between K and Q , it is necessary to make one further assumption. I assume, then, that $u_1 = \lambda v_1$ and $u_2 = \lambda v_2$, so that $V_1^2 = (\lambda^2 + 1)u_1^2$ and $V_2^2 = (\lambda^2 + 1)u_2^2$. I assume this because, if the streams did not interfere with one another at all, we should have

$$u_1^2 = \frac{1}{6} V_1^2;$$

so that, if $\lambda^2 + 1 = \alpha^2$, we should have

$$\alpha^2 = 6 \text{ and } \alpha = 2.5 \text{ } q. \text{ } p.$$

Our equations then become

$$V_1 - V_2^2 = \alpha^2 (u_1^2 - u_2^2);$$

$$\therefore Q = kK\alpha^2(u_1 - u_2).$$

From these we can eliminate u_1, u_2, ρ_1, ρ_2 ; and putting

$$V_1^2 - V_2^2 = X^2,$$

we get

$$Q^4 + 4 \frac{\alpha^2 k^2}{\rho} K^3 Q^2 - \alpha^2 k^4 X^4 \cdot K^4 = 0,$$

which is a quadratic for Q^2 or a biquadratic for K .

Solving for Q , we get

$$Q = \frac{kK\sqrt{\alpha}}{\sqrt{\rho}} \left\{ \sqrt{X^4 \rho^2 + 4\alpha^2 K^2} - 2\alpha K \right\}^{\frac{1}{2}},$$

as evidently the other solutions are inadmissible.

From this we may get an approximate value for K in terms of Q ; for, unless α be very large, or the density or difference of temperature very small, $X^2 \rho$ is much greater than $2\alpha K$. For instance, if V_1 and V_2 correspond to a difference of 10° C.,

$$V_1 = 48500 \sqrt{\frac{T_1}{273}},$$

$$V_2 = 48500 \sqrt{\frac{T_2}{273}},$$

and consequently

$$X^2 = \frac{(48500)^2}{27 \cdot 3};$$

$$\therefore X = 9700,$$

while $\rho = \frac{1}{800}$ for air at atmospheric pressure;

$$\therefore X^2 \rho = 107600.$$

And K would be large if it were 100; so that even if α were 50, $2\alpha K$ would still be less than $\frac{1}{10}$ of $X^2 \rho$; and so we may take approximately

$$Q = k\sqrt{\alpha} \cdot KX;$$

$$\therefore K = \frac{Q}{k\sqrt{\alpha} \cdot X}.$$

From this we can calculate K ; for $k = 1.6$ in most gases, and, if $\alpha = 2.5$, $\sqrt{\alpha} = 1.5$, and $X = 9700$, as above;

$$\therefore k\sqrt{\alpha}X = 22310 = 2 \times 10^4 q. p.$$

Now, at a distance of a fourth-metre in air at atmospheric pressure, and with a difference of temperature of 10° C.,

$$Q = 10^6 q. p.;$$

so that in this case

$$K = 50 q. p.,$$

which is within the limits of the quantities obtained in the case of the spheroidal drops on liquids.

That by this formula K varies nearly as Q , and not as Q^2 , is not to be wondered at, because in the first place the formula only professes to represent an approximation to the true state of affairs, and in the second place it is only at distances and pressures at which the ordinary laws of conduction of heat cease to apply that it professes even approximately to represent it.

The whole of these investigations are unsatisfactory to this extent—that I have been unable, from a consideration of the molecular encounters themselves, to discover what is the actual distribution of velocities even in the simple case of two parallel surfaces. This is hardly to be wondered at; for the problem is extremely complicated, and evidently depends upon the undecided point in molecular physics, namely the proportion of the molecules encountering in a given direction that are thrown off in the various other directions. We might very well assume, with Maxwell, that they are uniformly distributed in every direction after the encounter; but even this does not simplify the question sufficiently to bring it within my present powers of solution.

III. *The Electric Light.* By W. H. PREECE, *Memb. Inst. C.E., V.P. Soc. T.E., Electrician General Post-Office, &c.**

1. **T**HE theory of the electric light cannot be brought absolutely within the domain of quantitative mathematics, for the reason that we do not yet know the exact relation that exists between the production of heat and the emission of light with a given current; but we know sufficient to predicate that what is true for the production of heat is equally true for the production of light beyond certain limits.

The work done in a battery, or any source of current-electricity, is expended outside the battery in a closed circuit in the form of heat. When this heat acquires a certain temperature per unit mass, we have light. If the heat be confined to a mass of metal wire like platinum, we have light by *incandescence*; if it be expended in the transference of minute particles of incandescent matter like carbon across an air-space, we have the *electric arc*. The exact relations between current, heat, temperature, mass, and light have yet to be determined by experiment.

2. The arc is thus a form of energy developed in one point

* Communicated by the Author.

of a circuit, which is the exact equivalent of another form of energy expended in another point of the circuit. Thus, if we produce light by a galvanic battery, it is the equivalent of chemical work done in the battery. If it be produced by a dynamo-machine driven by a steam-engine, it is the equivalent of coal consumed in the furnace. The object to be attained in any economical utilization of this energy is to convert the greatest possible portion of it into light.

3. Now the relations that exist between the work done, the current flowing, the resistances present, and the heat developed are easily demonstrated. The work done (W) in any circuit varies directly with the electromotive force (E) in that circuit, and with the quantity of electricity (Q) that passes through it, or

$$W = EQ;$$

but by Ohm's law the electromotive force is equal to the product of the resistance (R) of the circuit into the current (C) flowing, or

$$E = CR;$$

and by Faraday's law the quantity of electricity passing depends upon the strength of current (C) and the time it flows (t), or

$$Q = Ct.$$

Therefore, substituting these two values in the above equation, we get

$$W = C^2Rt;$$

in which we have what is known as Joule's law, which gives us the work done (W), or its equivalent, the heat generated (H) in any circuit. By regarding the time as constant, we can put the equation

$$H = C^2R. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

4. Now let us take the case of a battery whose electromotive force is E and whose internal resistance is ρ . Let the resistance of the connecting wires be r . Let us also have a particular resistance l , which may be a wire to be heated to incandescence, or a lamp to be lit by the arc; then by Joule's law (1),

$$H = C^2(\rho + r + l);$$

but by Ohm's law,

$$C = \frac{E}{\rho + r + l},$$

$$\therefore H = \frac{E^2}{\rho + r + l}.$$

5. Confining our attention for the present to the heat generated (H), this will be distributed throughout the circuit; and that in the resistance (l) will be

$$H \times \frac{l}{\rho + r + l} = \frac{E^2 l}{(\rho + r + l)^2} \dots \dots (2)$$

Now if we suppose n resistances in circuit joined up in *series*, then the total heat generated will be

$$H' = \frac{E^2 n l}{(\rho + r + n l)^2} \dots \dots (3)$$

If we differentiate this fraction with respect to $n l$ and put it equal to nothing, we can find when the heat generated in these resistances becomes a maximum; that is,

$$\frac{dH'}{dn l} = \frac{1}{(\rho + r + n l)^4} [(\rho + r + n l)^2 E^2 - 2 E^2 n l (\rho + r + n l)] = 0,$$

whence

$$\rho + r + n l = 2 n l;$$

that is,

$$\rho + r = n l;$$

or the greatest heat is generated in the resistances when the value of the latter equals the resistances of the rest of the circuit.

6. Let us now assume the n resistances to be connected up in *multiple arc*; then the joint resistance will become $\frac{l}{n}$, and the heat generated will be

$$H'' = \frac{E^2 \frac{l}{n}}{(\rho + r + \frac{l}{n})^2}; \dots \dots (4)$$

and the maximum amount of heat will occur, as before, when

$$\rho + r = \frac{l}{n}.$$

7. Now, in the first case, if the internal resistance of the battery and of the connecting wires be very small compared with $n l$, we may neglect them; so that by putting $\rho + r = 0$, equation (3) becomes

$$H' = \frac{E^2}{n l};$$

or the total amount of heat generated in the resistances will vary inversely as the number of the latter in circuit.

8. In the second case, we cannot neglect $\rho + r$; for here the

greater we make n , the smaller $\frac{l}{n}$ becomes with respect to $\rho + r$; so that if eventually $\frac{l}{n}$ becomes very small, we may neglect it in the denominator of the fraction. Then

$$H'' = \frac{E^2 \frac{l}{n}}{(\rho + r)^2} = \frac{E^2 l}{n(\rho + r)^2}; \dots \dots (5)$$

so that in this case also *the total heat generated in the resistances will vary inversely as the number of the latter in circuit.*

9. Now it must be observed that in each of these cases the total heat is distributed over n resistances; and therefore, as compared with one resistance, the heat generated in each is only $\frac{1}{n^2}$ of that generated in one. So that, *joined up either in series or in multiple arc, the heat generated in each of a number of resistances varies inversely as the square of their number.*

10. With respect to the light emitted, if the amount of heat generated represented exactly the amount of light emitted, then the above equations would indicate the effects produced by multiplying the lights or subdividing the current when a constant battery is employed. But this is not so. The light obtained is not proportional to the heat generated. Below a certain limit the production of heat is not accompanied by light at all. In the case of incandescence, if the heat be distributed over two wires instead of one, inasmuch as the mass to be heated in the one case is double that in the other, the actual temperature to which each of the wires will be heated will be only one quarter of that obtained with one wire, and the *total* light emitted will be half what it was before. In the case of the arc a similar result probably takes place: the incandescent matter, which is heated by the current and which gives out the light, is increased by the addition of each lamp, and therefore diminishes the actual temperature of each arc, and consequently diminishes the light given out in direct proportion to the number of lights.

11. Moreover, in the arc the actual disintegration of the carbons and the transference of matter across the air-space, represent an amount of work done which must be deducted from that converted into heat, and which again tends to diminish the amount of light emitted. If, therefore, the lamps be joined up in series or in multiple arc, the light emitted by each lamp will vary inversely in a greater ratio *than* the square of the number in circuit.

12. We have assumed E to be constant; but if the current

be produced by a magneto- or dynamo-machine worked by a steam-engine consuming a given amount of coal per unit time, E is no longer constant, for it varies with the resistances in the circuit. The constant in this case is the work done in the steam-engine in unit time. Calling this W_1 , the total heat generated in the circuit when the lamps are joined up in series will be

$$H_1 = W_1 \times \frac{nl}{\rho + r + nl}; \quad \dots \quad (6)$$

and since the light varies inversely as n (§ 10), the light emitted

$$L_1 = W_1 \times \frac{nl}{n(\rho + r + nl)}; \quad \dots \quad (7)$$

and when joined up in multiple arc,

$$L_2 = W_1 \times \frac{l}{n \left(\rho + r + \frac{l}{n} \right)} \quad \dots \quad (8)$$

Or by putting $\rho + r = 0$ in equation (7), and $\frac{l}{n} = 0$ in the denominator of equation (8), we get

$$L_1 = \frac{W_1}{n}$$

and

$$L_2 = \frac{W_1 l}{(\rho + r) n^2}.$$

So that *beyond* certain limits, when the current is produced by a dynamo-machine, if n lamps be joined up in series, the total light becomes diminished by $\frac{1}{n}$, and the light emitted by each lamp becomes diminished by $\frac{1}{n^2}$.

If they are joined up in multiple arc, the total light is diminished by $\frac{1}{n^2}$, and the light emitted by each lamp $\frac{1}{n^3}$. In the latter case the rapid diminution in the light emitted is due to the fact that the heat is developed in the machine itself instead of in the resistances external to it.

13. We have assumed W_1 to be constant; but this is only the case when a certain limit is reached, and when the velocity of the rotating coils in the dynamo-machine has attained a

maximum. This limit will vary with each dynamo-machine and each kind of lamp used. With the Wallace-Farmer machine the limit appears to be reached when six lamps are connected up in series. With the Gramme alternating machine and Jablochhoff candles the limit appears to be five lamps. Beyond these limits the above laws will be true. It is this partial success in multiplying the light that has led so many sanguine experimenters to anticipate the ultimate possibility of its extensive subdivision—a possibility which this demonstration shows to be hopeless, and which experiment has proved to be fallacious*.

IV. *On the Inductions that occur in the Telephone.* By Professor H. F. WEBER†. (Communicated to the *Züricher naturforschenden Gesellschaft*, at the Meeting of July 1, 1878‡.)

M. DUBOIS-REYMOND has given, in his “Contribution to the Knowledge of the Telephone”§, the following theory of the inductions that take place in the telephone:—

The periodical variations of the electromagnetic potential P of the magnetic masses in the telephone in relation to the path of the current may, in the first approximation, be supposed proportional to the outbendings of the iron membrane. If the exciting membrane executes vibrations of the form $\Sigma A_m \sin(2\pi nmt)$, then the periodical variations of the electromagnetic potential are given by the expression

$$P - P_0 = \Sigma B_m \sin(2\pi nmt),$$

where P_0 denotes the value of P corresponding to the position of equilibrium of the membrane. M. Dubois-Reymond neglects the induction of the current-path upon itself as unessential, and sets forth as the really active electromotive force only

* *Vide* Fontaine’s ‘Electric Lighting,’ chapter xi.

† Translated from a separate impression, communicated by the Author, from the *Vierteljahrsschrift der Züricher naturforschenden Gesellschaft*.

‡ Ten days later, on the 11th July, 1878, M. Helmholtz transmitted to the Berlin Akademie der Wissenschaften a memoir in which he handled the same subject in the same manner. That already on the 1st July I had made known the contents of the present memoir is evidenced, *inter alia*, by the following, added by M. Hermann to his last paper in the *Annalen der Physik und Chemie*, new series, vol. v. p. 91, on the 2nd of July:—“Professor Fr. Weber, of Zurich, has succeeded in showing that the relation found by me is in harmony with the law of induction, and that the latter has been wrongly applied in the theory which I have controverted. He will shortly make a communication on this matter.”

§ *Archiv für Physiologie*, 1877, pp. 573, 582.

that which results from the value of P according to the general law of induction:

$$E = \frac{\partial P}{\partial t} = \Sigma B_m \cdot 2\pi nm \cdot \cos(2\pi nmt).$$

The current occasioned in the telephone is proportional to this quantity; and so is the outbending of the excited membrane in the receiving telephone. If the air-vibrations which excite the telephone have the form $\Sigma A_m \cdot \sin(2\pi mt)$, the vibrations excited in the air by the telephone take the form

$$\Sigma A_m^1 \cdot 2\pi nm \cdot \cos(2\pi nmt).$$

Thus the tone (*Klangfarbe*) of the sonorous motion, is necessarily altered during its telephonic transit: the partial tones of a higher number of vibrations come out stronger than those of a smaller number. At the same time there is a phase-displacement to the amount of $\frac{\pi}{2}$.

M. Hermann has shown, in his "Telephonic Notices"*, and in his last experiments, communicated to this Society, that these consequences of the theory of Dubois-Reymond are not verified by experience. Hermann instituted the following experiments:—

I. In the circuit of a telephone one of a pair of coils was inserted, and in that of a second telephone the other coil, parallel with the former. All the words and letters which were spoken into the first telephone could be heard distinctly out of the second. The same result still followed when a second, third, fourth pair of coils were in like manner inserted between the two telephones. From this M. Hermann infers that the induction is without any traceable influence upon the ratio of intensity of the partial tones of a sound; while, according to M. Dubois-Reymond's theory, in these cases of multiplied induction a very considerable alteration in the tone must have occurred.

II. In an induction-coil oscillating currents, I, were excited by a vibrating magnetic tuning-fork placed near it. These currents were conducted to a telephone. In the circuit of the currents I one set of windings of a double-wound electro-magnet-coil of fine wire was inserted. The currents I_1 , induced in the second set of turns of the coil, were also, a commutator and key being inserted, conducted to the telephone. Moreover the telephone could at pleasure be taken out of the circuit of

* Plüger's *Archiv für Physiologie*, vol. xvi. pp. 264 & 314.

the currents I or again inserted. One could thus investigate in succession the action of the currents I , the action of the currents I_1 , and the resultant action of the oppositely directed currents I and I_1 . It was found that the direction of the currents I_1 was always the opposite of that of the currents I , that the currents I_1 exerted a somewhat feebler action in the telephone than the currents I , that the resultant action of the currents when in the same direction amounted to nearly double the action of each singly, and that the resultant action of the oppositely directed currents was nearly equal to *nil*. M. Hermann concludes from these experiments that the phases of the oscillating currents I and I_1 cannot be displaced $\frac{\pi}{2}$ in relation to one another, that much rather they must nearly coincide.

M. Hermann has, he believes, made it evident by these experiments that neither the amplitudes nor the phases of the partial tones of a sound are sensibly altered by the induction that passes in the telephone, and, accordingly, that the above-stated inferences from the theory of M. Dubois-Reymond do not correspond with the facts; whether, and how, the results of his experiments can be brought into accordance with the general law of induction he leaves undecided.

The following communication is intended to show that the experimental results obtained by M. Hermann are in most perfect accordance with the general law of induction, and can be adduced as new and interesting evidence for the universal validity of that law. This perfect accordance is attained as soon as all the inductions that take place in the telephonic circuit are taken into account. Dubois-Reymond has neglected the induction of the telephonic circuit in regard to itself as unessential; it results from the following considerations that this induction is really the determining moment in the telephonic process.

Let a telephone be in a closed circuit, and a second telephone be inserted in another closed circuit; let both circuits be so constituted and placed that they exert upon each other a powerful reciprocal induction.

(a) For that one of the two closed circuits which contains the exciting telephone T , let

W signify the resistance of the circuit,

I the current-intensity of this circuit,

Q the electrodynamic potential of this circuit upon itself,

P the electromagnetic potential of the magnetic masses in the telephone T referred to the conduction of the current;

(b) For the other closed circuit, which contains the excited telephone T, let

W_1 signify the resistance of the circuit,

I_1 the intensity of the current excited,

Q_1 the electrodynamic potential of this circuit upon itself.

Lastly, let R be the reciprocal electrodynamic potential of the two circuits.

The general law of induction furnishes, for the determination of the two current-intensities I and I_1 , the two equations

$$\left. \begin{aligned} I \cdot W &= \frac{\partial P}{\partial t} - Q \frac{\partial I}{\partial t} - R \frac{\partial I_1}{\partial t}, \\ I_1 \cdot W_1 &= - Q_1 \frac{\partial I_1}{\partial t} - R \frac{\partial I}{\partial t}. \end{aligned} \right\} \dots (1)$$

Let the electromagnetic potential P have the form

$$P = P_0 + A \sin(2\pi n t).$$

The equations

$$I = C \sin(2\pi n t + \alpha),$$

$$I_1 = C_1 \sin(2\pi n t + \alpha_1),$$

satisfy the equations (1), if the amplitudes C and C_1 have the following values,

$$C = \frac{A}{Q} \sqrt{\frac{1 + \left(\frac{W_1}{2\pi n Q_1}\right)^2}{\left[\frac{W}{2\pi n Q} + \frac{W_1}{2\pi n Q_1}\right]^2 + \left[1 - \frac{R^2}{Q Q_1} - \frac{W W_1}{(2\pi n)^2 Q Q_1}\right]^2}} \quad (2)$$

$$C_1 = - \frac{A \cdot R}{Q Q_1 \sqrt{\left[\frac{W}{2\pi n Q} + \frac{W_1}{2\pi n Q_1}\right]^2 + \left[1 - \frac{R^2}{Q Q_1} - \frac{W W_1}{(2\pi n)^2 Q Q_1}\right]^2}} \quad (3)$$

and to the phases α and α_1 the following values be given,

$$\tan \alpha = \frac{\left[1 + \left(\frac{W_1}{2\pi n Q_1}\right)^2\right] \frac{W}{2\pi n} + \frac{R^2}{Q_1^2} \frac{W_1}{2\pi n}}{1 + \left(\frac{W_1}{2\pi n Q_1}\right)^2 Q - \frac{R^2}{Q_1}}, \quad \dots (4)$$

$$\tan \alpha_1 = \frac{\frac{W_1}{2\pi n Q_1} + \frac{W}{2\pi n Q}}{1 - \frac{R^2}{Q Q_1} - \left(\frac{W}{2\pi n Q}\right) \left(\frac{W_1}{2\pi n Q_1}\right)} \quad \dots (5)$$

If both telephones be in one and the same circuit, of which the resistance is W_0 , and the electrodynamic potential upon

itself is Q_0 , then we get from equation (2), as the expression of the amplitude of the resulting oscillating current,

$$C_0 = \frac{A}{Q_0 \sqrt{1 + \left(\frac{W_0}{2\pi n Q_0}\right)^2}}; \dots \dots (6)$$

and the phase α_0 is in this case determined by the equation, resulting from (4),

$$\tan \alpha_0 = \frac{W_0}{2\pi n Q_0} \dots \dots \dots (7)$$

The results obtained show:—

(1) In the telephonic transit the tone is in general altered, since the amplitude of the oscillating current C_1 (and C_0 respectively) is dependent on the number of vibrations n of the exciting potential P —that is, on the vibration-number of the exciting simple tone.

(2) The phase-displacement that occurs during the telephonic transit is not a constant quantity; its amount changes with the constitution of the path of the current, and depends on the number n of the vibrations.

(3) In certain cases, however, the amplitude C_1 (and C_0 respectively) of the induced current becomes independent of the vibration-number n , and thus the tone of the exciting sound is unchanged. This occurs as soon as the quantities

$$\frac{W}{2\pi n Q} \text{ and } \frac{W_1}{2\pi n Q_1}$$

come out so small that their second dimensions can be neglected against 1. The resultant values for C , C_1 , and C_0 in this case are

$$C = \frac{A Q_1}{Q Q_1 - R^2}, \quad C_1 = \frac{A \cdot R}{Q Q_1 - R^2}, \quad C_0 = \frac{A}{Q}.$$

Under these circumstances the phases are determined by the equations

$$\tan \alpha = \frac{\frac{W}{2\pi n} Q_1 + \frac{R^2}{Q_1} \frac{W_1}{2\pi n}}{Q Q_1 - R^2},$$

$$\tan \alpha_1 = \frac{\left(\frac{W_1}{2\pi n}\right) Q + \left(\frac{W}{2\pi n}\right) Q_1}{Q Q_1 - R^2}$$

and

$$\tan \alpha_0 = \frac{W_0}{2\pi n Q_0}.$$

The phase-displacements α , α_1 , α_0 are accordingly small quantities of the same order, which in the limiting case

$$\frac{W}{2\pi nQ} = \frac{W_1}{2\pi nQ_1} = \frac{W_0}{2\pi nQ_0} = 0$$

become $=0$.

But the results deduced for this special case are the expression of those obtained experimentally by M. Hermann. If it can be shown that in fact the quantities $\frac{W}{2\pi nQ}$ and $\frac{W_1}{2\pi nQ_1}$ were very small values in his experiments, then the complete agreement of the results of experiment with the general law of induction is demonstrated. By a closer consideration of the dimensions, the number of turns, and the resistances of the coils and telephones employed by him, we can perceive that the quotients mentioned actually were small values.

V. Starch and Unannealed Glass under the Polariscope.

By WALTER BAILY*.

[Plates I.-IV.]

THERE are many bodies, of which a spherical grain of starch and a circular plate of unannealed glass may be taken as specimens, having an optical structure symmetrical about an axis through the body. The object of this paper is to investigate the state of the light which emerges from such a body, when monochromatic light in any state of polarization is sent through the body in the direction of the axis.

In fig. 1 (Plate I.) let SS' and TT' be drawn through R perpendicular to one another, and let UU' and VV' bisect the angles between them. Suppose a quarter-undulation plate to be fixed parallel to the paper, with its axes parallel to SS' and TT' —and the light to be passed perpendicularly to the paper through a Nicol's prism having its axis perpendicular to the paper and its plane of polarization inclined at an angle ρ to the line SS' , then through the quarter-undulation plate, and then through the body, which is also to be placed with its axis perpendicular to the paper.

Let the paper represent a section of the light after it has emerged from the body. Take any point P and draw round it an ellipse representing the polarization of the light at P . The state of the light will be completely determined if we know the angle (α) which the axis major of this ellipse makes with RP , the angle (β) which a line joining the extremities

* Read before the Physical Society, June 20, 1878.

of the axes of the ellipse makes with the axis major, and the direction in which the rotation takes place.

Let the angle $\text{SRP} = \phi$; produce RP to X and draw $\text{PY} \perp \text{PX}$; take any point Q on the ellipse, let $\text{PQ} = r$, and the angle $\text{QPX} = \theta$.

The resolved part of the vibration at P along RP has been retarded in passing through the body differently to the resolved part perpendicular to RP . Let the resolved part along RP have been retarded by a quantity σ more than the mean amount, and the other part have been retarded by the same quantity less than the mean amount. The quantity σ is a function of the distance RP .

Let $\sin t$ represent the vibration in the æther after the light has passed through the Nicol. This is equivalent to

$$\cos \rho \sin t \quad \parallel \text{SS}'$$

and

$$\sin \rho \sin t \quad \parallel \text{TT}'.$$

After passing through the quarter-undulation plate the vibrations become

$$\cos \rho \sin (t + 45^\circ) \quad \parallel \text{SS}'$$

and

$$\sin \rho \sin (t - 45^\circ) \quad \parallel \text{TT}'.$$

These may be written (the coefficient $\frac{1}{\sqrt{2}}$ being omitted)

$$\cos \rho (\sin t + \cos t) \quad \parallel \text{SS}'$$

and

$$\sin \rho (\sin t - \cos t) \quad \parallel \text{TT}'.$$

It is easy to show that these vibrations represent motion in an ellipse whose axes are parallel to SS' and TT' , and the extremities of whose axes are joined by a line making an angle ρ with the axis major.

These vibrations are equivalent to

$$\cos (\rho - \phi) \sin t + \cos (\rho + \phi) \cos t \quad \parallel \text{PX}$$

and

$$\sin (\rho - \phi) \sin t - \sin (\rho + \phi) \cos t \quad \parallel \text{PY}.$$

After passing through the body the displacement $\parallel \text{PX}$ is $r \cos \theta$, and that $\parallel \text{PY}$ is $r \sin \theta$. Hence

$$r \cos \theta = \cos (\rho - \phi) \sin (t + \sigma) + \cos (\rho + \phi) \cos (t + \sigma),$$

$$r \sin \theta = \sin (\rho - \phi) \sin (t - \sigma) - \sin (\rho + \phi) \cos (t - \sigma);$$

which may be written

$$r \cos \theta = a \sin t + b \cos t, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$r \sin \theta = a' \sin t + b' \cos t, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$\begin{aligned} a &= \cos(\rho - \phi) \cos \sigma - \cos(\rho + \phi) \sin \sigma, \\ b &= \cos(\rho - \phi) \sin \sigma + \cos(\rho + \phi) \cos \sigma, \\ a' &= \sin(\rho - \phi) \cos \sigma - \sin(\rho + \phi) \sin \sigma, \\ b' &= -\sin(\rho - \phi) \sin \sigma - \sin(\rho + \phi) \cos \sigma. \end{aligned}$$

Differentiating (1) and (2) with respect to t , we have

$$\cos \theta \frac{dr}{dt} - \sin \theta r \frac{d\theta}{dt} = a \cos t - b \sin t, \quad . \quad . \quad (3)$$

$$\sin \theta \frac{dr}{dt} + \cos \theta r \frac{d\theta}{dt} = a' \cos t - b' \sin t. \quad . \quad . \quad (4)$$

Multiplying (3) by $r \sin \theta$, and (4) by $r \cos \theta$, and subtracting, we have

$$\begin{aligned} r^2 \frac{d\theta}{dt} &= (a \sin t + b \cos t)(a' \cos t - b' \sin t) \\ &\quad - (a' \sin t + b' \cos t)(a \cos t - b \sin t); \end{aligned}$$

whence

$$\begin{aligned} r^2 \frac{d\theta}{dt} &= a'b - ab' \\ &= \sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi. \quad . \quad (5) \end{aligned}$$

Again, in (1), (2), (3), (4), let t have a value which makes r a maximum or minimum; then θ becomes α , and $\frac{dr}{dt}$ vanishes.

Eliminating r from (1) and (2), we get

$$(a \sin \alpha - a' \cos \alpha) \sin t = -(b \sin \alpha - b' \cos \alpha) \cos t;$$

and eliminating $r \frac{d\theta}{dt}$ from (3) and (4), we get

$$(b \cos \alpha + b' \sin \alpha) \sin t = (a \cos \alpha + a' \sin \alpha) \cos t.$$

Hence

$$\frac{a \sin \alpha - a' \cos \alpha}{b \cos \alpha + b' \sin \alpha} + \frac{b \sin \alpha - b' \cos \alpha}{a \cos \alpha + a' \sin \alpha} = 0,$$

or

$$2(aa' + bb') - (a^2 + b^2 - a'^2 - b'^2) \tan 2\alpha = 0;$$

that is,

$$\sin 2\rho \sin 2\sigma + \cos 2\rho \cos 2\sigma \sin 2\phi + \cos 2\rho \cos 2\phi \tan 2\alpha = 0. \quad (6)$$

Now $r^2 \frac{d\theta}{dt}$ is proportional to the area of the ellipse, as the period of vibration is constant; and the axes of the ellipse are proportional to $\sin \beta$ and $\cos \beta$, since the intensity of the light, and consequently the sum of the squares of the axes is constant over the whole section. Hence the area of the ellipse is also proportional to $\sin \beta \cdot \cos \beta$ (that is, to $\sin 2\beta$). Putting

$\rho=45^\circ$ and $\sigma=0$ in (5), we get $r^2 \frac{d\theta}{dt} = 1$. But in this case the emerging light is circularly polarized, and consequently $\beta=45^\circ$, whence $\sin 2\beta$ also equals unity. Hence in every case

$$r^2 \frac{d\theta}{dt} = \sin 2\beta.$$

Hence

$$\sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi - \sin 2\beta = 0. \quad (7)$$

The locus of points at which the major axis of the ellipse of polarization is inclined at a constant angle to the radius RP will be called an "isoclinal line," and will be denoted by the symbol $K(\alpha)$, where α is this angle. $K(\alpha)$ and $K(\alpha-90^\circ)$ are both included in the equation (6), as that equation does not distinguish between major and minor axes.

The locus of points at which the line joining the extremities of the axes makes a constant angle with the major axis will be called an "isomorphal line," and will be denoted by the symbol $M(\beta)$, where β is this angle. Equation (7) is the equation to $M(\beta)$.

The direction of rotation of the æther is positive or negative according as $\sin 2\beta$ is positive or negative—that is, according as β is positive or negative, since we need only give β values lying between $\pm 45^\circ$.

In figs. 2, 3, 4, 5 these loci are represented—the isomorphals by continuous lines, and the isoclinals by dotted lines. The Arabic numerals indicate the values of α in degrees, and the Roman numerals those of β in degrees.

The value given to σ is $RP + 90^\circ$, a function which has been assumed solely for convenience in drawing the figures. The figures are drawn between the limits $\sigma=180^\circ$ and $\sigma=360^\circ$. An extension beyond these limits would give merely a repetition of the portion within them, since an addition of 180° to the value of σ makes no difference in the equations to the loci. Directions from the centres of these figures will be referred to by means of the lines in fig. 1.

The values of ρ are in fig. 2, 45° , in fig. 3, 30° , in fig. 4, 15° , and in fig. 5, zero. Accordingly the incident light is circularly polarized in fig. 2, elliptically in figs. 3 and 4 (the eccentricity of the ellipse being less in fig. 3 than in fig. 4), and plane-polarized in fig. 5.

The isomorphals and isoclinals drawn are those for which the values of α and β are 45° , 30° , 15° , and zero. The thick continuous lines are branches of $M(0)$, which is the locus of plane-polarized light. The round spots are loci of circularly polarized light, or $M(\pm 45)$.

The symbol $K(\pm\alpha)$ will be used to include $K(\alpha)$, $K(\alpha-90^\circ)$, $K(-\alpha)$, and $K(90^\circ-\alpha)$; its equation is

$$(\sin 2\rho \sin 2\sigma + \cos 2\rho \cos 2\sigma \sin 2\phi)^2 = \cos^2 2\rho \cos^2 2\phi \tan^2 2\alpha. \quad (8)$$

And the symbol $M(\pm\beta)$ will be used to include $M(\beta)$ and $M(-\beta)$; its equation is

$$(\sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi)^2 = \sin^2 2\beta. \quad (9)$$

Putting $90^\circ-\phi$, or $-90^\circ-\phi$, for ϕ in these equations makes no change in the equations. Hence $K(\pm\alpha)$ and $M(\pm\beta)$ are each symmetrical with respect to UU' and VV' .

Adding (8) and (9), we get

$$\cos^2 2\phi \cdot \cos^2 2\rho = \cos^2 2\alpha \cdot \cos^2 2\beta, \quad \dots \quad (10)$$

from which equation it appears that the intersections of $K(\pm\alpha)$ with $M(\pm\beta)$ all lie on the two diameters defined by the equation; and since α and β are interchangeable in the equation, we see that $K(\pm\gamma)$ and $M(\pm\delta)$ intersect on the same two diameters at $K(\pm\delta)$ and $M(\pm\gamma)$.

Let

$$\sigma = \left(\frac{n}{2} + \frac{1}{4}\right)\pi + \mathfrak{S};$$

then $K(\pm\alpha)$ becomes

$$(\sin 2\rho \cos 2\mathfrak{S} - \cos 2\rho \sin 2\mathfrak{S} \sin 2\phi)^2 = \cos^2 2\phi \cos^2 2\rho \tan^2 2\alpha,$$

and $M(\pm\beta)$ becomes

$$(\sin 2\rho \sin 2\mathfrak{S} + \cos 2\rho \cos 2\mathfrak{S} \sin 2\phi)^2 = \sin^2 2\beta.$$

From these equations it appears that, in changing the sign of ϕ , we shall change only the sign and not the magnitude of \mathfrak{S} ; so that if we have drawn part of $K(\pm\alpha)$ or $M(\pm\alpha)$ between the limits $\phi=0$ and $\phi=45^\circ$, we can draw the corresponding part of the curve between the limits $\phi=0$ and $\phi=-45^\circ$. If σ' be the value of σ corresponding to $-\phi$,

$$\sigma' = \left(\frac{n}{2} + \frac{1}{4}\right)\pi - \mathfrak{S},$$

and therefore

$$\sigma + \sigma' = \left(n + \frac{1}{2}\right)\pi. \quad \dots \quad (11)$$

Putting $\alpha=0$ and $\beta=0$ in (8) and (9), we get for $K(0)$,

$$\sin 2\rho \sin 2\sigma + \cos 2\rho \cos 2\sigma \sin 2\phi = 0; \quad \dots \quad (12)$$

and for $M(0)$,

$$\sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi = 0. \quad \dots \quad (13)$$

Let s be the value of σ where a radius ϕ intersects a branch

of $M(0)$, and let $s + \xi$ be the value of σ where the same radius intersects a branch of $K(\pm\alpha)$; then by (8) we have

$$\begin{aligned} & \{(\sin 2\rho \sin 2s + \cos 2\rho \cos 2s \sin 2\phi) \cos 2\xi \\ & \quad + (\sin 2\rho \cos 2s - \cos 2\rho \sin 2s \sin 2\phi) \sin 2\xi\}^2 \\ & = \cos^2 2\rho \cos^2 2\phi \tan^2 2\alpha. \quad \dots \dots \dots (14) \end{aligned}$$

But by (13) the coefficient of $\sin 2\xi = 0$, and ξ is determined by $\cos 2\xi$ only, from which it appears that ξ has pairs of values of equal magnitude and opposite sign. Hence, if we have drawn a part of $K(\pm\alpha)$ on one side of a branch of $M(0)$, we can draw the corresponding part on the other side of $M(0)$.

A similar property of $M(\pm\beta)$ with respect to $K(0)$ may be proved in the same manner.

Where $K(\pm\alpha)$ intersects $M(0)$, ξ vanishes and the diameter becomes a tangent. Its position is determined by putting $\beta=0$ in (10). This gives

$$\cos^2 2\phi \cos^2 2\rho = \cos^2 2\alpha. \quad \dots \dots \dots (15)$$

The position of the diameter tangents to $M(\pm\beta)$ is determined by putting $\alpha=0$ in the same equation. We get

$$\cos^2 2\phi \cos^2 2\rho = \cos^2 2\beta. \quad \dots \dots \dots (16)$$

Hence the same diameters are tangents to $K(\pm\gamma)$, $M(\pm\gamma)$ at the points where they intersect $M(0)$ and $K(0)$ respectively.

If we put $\sigma \pm 45^\circ$ for σ in $K(0)$ we obtain $M(0)$, and conversely. See equations (12) and (13). $\dots \dots \dots (17)$

If we put $\sigma \pm 90^\circ$ for σ in $K(\pm\alpha)$ or $M(\pm\beta)$, the equation is unaltered; and by this means from one branch of one of these curves we can obtain all other branches of the curve. (18)

We will now consider the forms of the loci, dealing first with figs. 3 and 4, in which the incident light is elliptically polarized.

To obtain the isomorphals, we have the equation (7) for $M(\beta)$ and (13) for $M(0)$.

Putting $\beta = \rho$, we obtain the following equation to $M(\rho)$, viz.

$$\sin \sigma (\sin 2\rho \sin \sigma + \cos 2\rho \cos \sigma \sin 2\phi) = 0; \quad \dots (19)$$

and putting $\beta = -\rho$, we obtain the following equation to $M(-\rho)$, viz.

$$\cos \sigma (\sin 2\rho \cos \sigma - \cos 2\rho \sin \sigma \sin 2\phi) = 0. \quad \dots (20)$$

$M(\rho)$ therefore consists of circles for which $\sin \sigma = 0$, and ovals intersecting those circles on the diameters SS' and TT' ; and $M(-\rho)$ consists of circles for which $\cos \sigma = 0$, and ovals intersecting those circles also on SS' and TT' . The outer circles in the figures are circles of $M(\rho)$; and the middle circle is one of the circles of $M(-\rho)$.

When $\beta = \pm 45^\circ$, the light becomes circularly polarized, and therefore the value of α becomes indefinite; consequently in equation (6) we must have $\cos 2\phi = 0$. Putting this value of $\cos 2\phi$ in (7), we get the following values of ϕ , σ , and β at points where the polarization is circular:—

ϕ .	σ .	β .	
$+45^\circ$,	$n\pi + 45^\circ + \rho$,	-45°	} . (21)
$+45^\circ$,	$n\pi + 135^\circ + \rho$,	$+45^\circ$	
-45° ,	$n\pi + 45^\circ - \rho$,	$+45^\circ$	
-45° ,	$n\pi + 135^\circ - \rho$,	-45° .	

These points will be called the “circular points.” All the isoclinals pass through all the circular points. The sign of β indicates the direction of rotation of the æther. Where the sign is positive, the direction of rotation has not been altered by the passage of the light through the body; where the sign is negative, the direction of rotation has been reversed.

To draw the isomorphals:—

Mark the circular points by (21). Draw the circles of $M(\rho)$ and $M(-\rho)$ by (19) and (20). Draw one branch of $M(0)$ from $\phi = 0$ to $\phi = 45^\circ$ by (13).

Obtain a branch of $K(0)$ by (17).

Obtain an oval of $M(\rho)$ between the above limits by (14).

Draw the part of a branch of $M(\beta)$, for example, $M(XV.)$ in fig. 3, or $M(XXX.)$ in fig. 4, which lies on one side of $M(0)$, and complete on the other side by (14).

Draw the remaining branches of $M(\pm\beta)$ between the above limits by (18).

Draw the isomorphals between $\phi = 0$ and $\phi = -45^\circ$ by (11), and complete the figure by means of the symmetry about UU' and VV' .

Write against the isomorphals the values of β , taking care to make the sign of β the same as that at the nearest circular point.

To draw the isoclinals:—

Equations (6) and (8) are not in a form available for calculation; but by solving (8) as a quadratic in $\sin 2\phi$, we obtain the equation to $K(\pm\alpha)$ in the form

$$\sin 2\rho \cos 2\sigma \sin 2\sigma \cos^2 2\alpha + \cos 2\rho (1 - \sin^2 2\sigma \cos^2 2\alpha) \sin 2\phi = \pm \sin 2\alpha (\cos^2 2\rho - \sin^2 2\sigma \cos^2 2\alpha)^{\frac{1}{2}}, \quad (22)$$

from which, by putting successive values for σ , we can obtain corresponding value of ϕ .

By putting $\alpha = \rho$, we obtain the equation to $K(\pm\rho)$, viz.

$$\sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi = \pm \sin 2\phi. \quad (23)$$

We have already obtained a branch of $K(0)$. Draw from (23) the part of a branch of $K(\rho)$ which lies on one side of $M(0)$ between the limits $\phi=0$ and $\phi=45^\circ$, and from (22) obtain a similar part of a branch of $K(\pm\alpha)$ —for example, $K(15)$ in fig. 3 and $K(30)$ in fig. 4.

Complete these branches by (14).

Complete the other branches between the same limits by (18).

Draw the figure between $\phi=0$ and $\phi=-45^\circ$ by (11), and complete the figure by the symmetry about UU' and VV' .

Draw straight lines in the direction UU' and VV' to give $K(\pm 45)$, for which the equation is

$$\sin 2\phi = 0. \quad . \quad . \quad . \quad . \quad . \quad (24)$$

To number the isoclinals:—

Note in (6) that when $\sigma = n\pi$, $\tan 2\alpha = -\tan 2\phi$, or $\alpha = -\phi$; and when $\sigma = (n + \frac{1}{2})\pi$, $\tan 2\alpha = \tan 2\phi$, or $\alpha = +\phi$. Hence the intersections of the isoclinals with the circles of $K(\rho)$ graduate that circle in the negative direction, and their intersections with the circle of $K(-\rho)$ graduate that circle in the positive direction. Graduate these circles accordingly, taking care to deduct 180° from the graduation when it exceeds 90° , and 360° from it when it exceeds 270° , and the readings will give the values of α .

The deductions are made to keep the readings low, and for the sake of symmetry.

From the figures 3 and 4 it appears that $M(\pm\rho)$ divides the figure into regions of two kinds: one kind, which I will call the “segments,” contains all the points at which the light is more circularly polarized than the incident light; and the other kind, which I will call the “rings,” contains all the points at which the light is more plane-polarized.

In the segments the isomorphals are closed curves surrounding the circular points; and in the rings the isomorphals are closed curves surrounding the centre of the figure.

It also appears that $K(\pm\rho)$ divides the figure into regions of two kinds—one containing all the points at which both the axes of the ellipse are inclined to the radius by a greater angle than ρ , and the other containing all the points at which one of the axes is inclined at a less angle than ρ . Both kinds of regions are four-cornered, and have two opposite corners on circular points; but in the first kind both these circular points lie on the same radius, and in the second the circular points lie on different radii. The isoclinals in each region pass from one circular point to the other.

Comparing fig. 3 with fig. 4, we see that as ρ increases, the segments become smaller, and the isomorphals in the rings become more circular. When $\rho = 45^\circ$, as in fig. 2, the segments vanish, and the isomorphals all become circular, the equation to $M(\beta)$ becoming

$$\cos 2\sigma - \sin 2\beta = 0.$$

The circular points are retained in the figure to show its relation to the other figures; but the whole circles through them are loci of circularly polarized light.

The equation to $K(\pm\alpha)$ gives $\tan 2\alpha = \infty$;

$$\therefore \alpha = \pm 45^\circ.$$

The equation to $K(\pm\rho)$ becomes

$$\cos 2\sigma = \pm \sin 2\phi.$$

This curve is retained in the figure to show the continuity with the other figures—although, as the inclination is everywhere 45° or -45° , the points on the curve have no special properties. For the same reason the straight lines $K(\pm 45)$ are retained.

Again, comparing figs. 3 and 4, we see that as ρ diminishes, the segments increase; and at last, in fig. 5, where ρ vanishes, the segments fill the whole space and the rings vanish. Each closed curve of $M(0)$ is forced into a broken line consisting of quadrants of circles joined by pieces of diameters; and as these closed curves now touch at their angles, they form together a complete system of circles and diameters whose equation is, putting $\rho = 0$ in (13),

$$\sin 2\sigma \sin 2\phi = 0.$$

The isoclinal $K(0)$ also forms a system of circles and diameters; their equation is, from (12),

$$\cos 2\sigma \sin 2\phi = 0.$$

All the other isomorphals form closed curves round the circular points; their equation is, from (7),

$$\sin 2\sigma \sin 2\phi + \sin 2\beta = 0.$$

And all the other isoclinals pass from one circular point to another on the same radius; their equation is, from (6),

$$\cos 2\sigma \tan 2\phi + \tan \alpha = 0.$$

The diameters of $K(0)$ and $M(0)$ coincide; this is indicated

in fig. 5 by the thick continuous line having dots on one side of it.

The figure may be drawn in a similar manner to that described for figs. 3 and 4.

The locus of plane-polarized light may be investigated without reference to the condition of the rest of the light, by drawing $M(0)$ by equation (13), and marking the direction of polarization at successive points on it by equation (15).

In figs. 6, 7, and 8 the points through which short straight lines are drawn are points at which the light is plane-polarized; and the short straight lines through them show the direction of polarization at the points. The dotted lines connecting these points are loci of plane-polarized light. The centres about which small circles are drawn are points at which the light is circularly polarized; and in fig. 6 the dotted lines connecting these circles are loci of circularly polarized light. The signs within the circles indicate the direction of rotation of the æther; see (21).

In fig. 6, $\rho = 45^\circ$; in fig. 7, $\rho = 22^\circ 30'$; in fig. 8, $\rho = 0$.

If the light be passed through an analyzing Nicol with its plane of polarization inclined at an angle ρ' to SS' , we can obtain the intensity of the light at any point as follows:—

The vibration along the major axis is $\cos \beta \cos t$, and that along the minor axis is $\sin \beta \sin t$; so that the vibration in the direction of the plane of polarization of the analyzer is

$$\cos(\rho' - \alpha) \cos \beta \cos t - \sin(\rho' - \alpha) \sin \beta \sin t.$$

Hence, if I is the intensity of the light after passing the analyzer,

$$I = \cos^2(\rho' - \alpha) \cos^2 \beta + \sin^2(\rho' - \alpha) \sin^2 \beta,$$

$$2I = 1 + \cos 2(\rho' - \alpha) \cos 2\beta.$$

The appearance of the light after passing an analyzer might be calculated from this equation, but can be inferred more readily by an inspection of the figures, which show its state before passing.

We notice that two dark spots will be seen on each branch of $M(0)$, one at each extremity of a diameter, at the points where the vibration is perpendicular to the plane of the analyzer. The spots on the successive branches of $M(0)$ will be alternately on a certain diameter and on the diameter perpendicular to it.

When the incident light is circularly polarized, these spots will move round in circles with unaltered appearance and at a uniform rate as the analyzer is turned uniformly. See figs. 2 and 6.

When the incident light is elliptically polarized, the spots will move round the curves $M(0)$; but the rate and appearance will vary (see figs. 3, 4, and 7). For on the circles of $M(\rho)$ the major axis of the ellipse preserves a constant direction in space, since $\phi + \alpha = 0$; but on the circles of $M(-\rho)$ the major axis rotates uniformly in space with an angular velocity double that of the radius, since $\phi - \alpha = 0$. Hence in those portions of $M(0)$ which are near the circles of $M(\rho)$, the change in the direction of the vibration will be slow; so that in this part the spot will be elongated, and will move more rapidly than the analyzer is rotated: but in the parts of $M(0)$ near the circles of $M(-\rho)$, the change in the direction of the vibration will be rapid; so that in these parts the spot will be shortened, and will move more slowly than the analyzer is rotated.

When the incident light is plane-polarized (see figs. 5 and 8), the slow-changing parts of $M(0)$ have combined to form the inner and outer circles of the figure and the diameters SS' and TT' . Along these lines the direction of vibration has no change, but remains constantly the same as that of the incident light; but on the middle circle of the figure, and on corresponding circles, the direction of vibration rotates uniformly with a velocity double that of the radius. Hence on the latter circles there will be spots moving uniformly round with a velocity double that of the analyzer; but on the other parts of the figure there will be no spots. However, when the spots on the latter circles reach the diameters, then the former circles and the diameters will become black.

If the light is not monochromatic, these appearances will not be so distinctly seen, as the absence of one colour will not occur exactly in the same place as the absence of another, since the position of the isomorphical and isoclinal lines depends upon the value of σ , and this will differ for different colours. But the position of diameters which give plane-polarized light in figs. 5 and 8, is not dependent on the value of σ ; and hence with any light this cross will always appear uncoloured, being black when the upper and lower Nicols are crossed, and in full light when they are parallel.

If σ is constant, the isomorphals and isoclinals become straight lines from the centre, and the state of the polarization may be conveniently represented by taking a series of points in a circle round the centre, and drawing about each point the ellipse of polarization at that point. The ellipse will show the polarization along the radius on which it lies.

This is done in figures 9 to 13, in which σ is about 15° . In fig. 9, $\rho = 45^\circ$; in fig. 10, ρ lies between 45° and σ ; in fig. 11, $\rho = \sigma$; in fig. 12, ρ lies between σ and zero; and in fig. 13, ρ is zero.

Suppose now the light to be passed through an analyzer placed with its plane of polarization in the direction TT'. When $\rho = 45^\circ$, the quadrants about UU' will be dark, and those about VV' will be light. They will gradually shade into one another, there being no black or full light. As ρ diminishes, UU' becomes darker until, when $\rho = \sigma$, UU' is black (see fig. 11). As ρ further diminishes, the black bar opens out into a dark oblique cross, neither bar of which is black; and when ρ becomes zero, this cross becomes rectangular and black. As ρ passes on to $-\sigma$, the cross becomes oblique and not black, and closes up into a black bar along VV'; and when ρ becomes -45° , the quadrants about VV' are dark, and those about UU' light. When we have the oblique cross, we can by a suitable turn of the analyzer make either arm of the cross black. (See fig. 12.)

If the analyzer is placed with its plane of polarization in the direction SS', we get the same set of appearances, except that we get light for dark and dark for light; and in the case of the single bar and rectangular cross, we get full light instead of black.

The appearances presented when σ is variable may be well seen in cylindrical disks of unannealed glass. I do not know of any bodies which show very clearly the appearances presented when σ is constant. Crystals of salicene show the black cross remarkably well, and give indications of the single black bar; but in this substance σ , though constant along each radius, varies in passing from one radius to another, and this completely hides the phenomena of the oblique cross. However, in grains of *tous-les-mois* starch, phenomena closely analogous to those above described as presented when σ is constant may be easily observed under a moderately high power—the only difference in the phenomena being that, in consequence of the grain of starch being generally an unsymmetrical body, the lines are distorted, the black cross, for instance, being neither rectangular nor rectilinear. See "The Optical Properties of Starch," Phil. Mag. for August 1876.

VI. *A new Proposition in the Theory of Diffraction, and its Application.* By J. FRÖHLICH, of Budapest.

THE mathematical expression which serves for the calculation of the intensity of diffracted light leads, after a few simple reasonings, to a peculiar connexion between the kinetic energy of the diffracted light issuing from an element of a luminous surface and incident upon an infinitely large receiving screen, on the one hand, and, on the other, the energy of the light proceeding from a very large luminous surface and yet incident upon only one element of the screen.

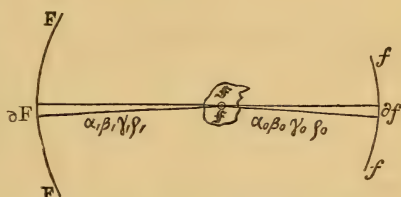
This relation is specially note-worthy on account of its facility of application; for it gives a simple method of observation which permits the question of the equality of the kinetic energy of the incident and the diffracted light to be at once decided experimentally for any aperture.

For the deduction of this proposition we make the following suppositions:—

Let there be a diffracting aperture \mathfrak{F} (fig. 1), bounded by any plane- or space-curve,

Fig. 1.

the dimensions of which relative to the wave-length of the light are very great, so that the amplitude of the diffracted light possesses a finite value only when the angle of diffraction is small, but otherwise vanishes. Further, let there be a uniformly luminous source of light of the form of a spherical surface FF , from which the light, after diffraction by \mathfrak{F} , arrives at the likewise spherical-surface-shaped receiving screen ff . The radii of the surfaces FF and ff , ρ_1 and ρ_0 , are very long in proportion to the dimensions of the aperture; and their common centre O lies in the aperture or its immediate vicinity.



Let the amplitude of the light-motion which emanates from the element ∂F be, at unit distance, $(\mathfrak{A}_0^2 \partial F)^{\frac{1}{2}}$ (since each element ∂F vibrates quite independently of the rest); consequently there arrives at the diffracting aperture a motion the amplitude of which is $\frac{(\mathfrak{A}_0^2 \partial F)^{\frac{1}{2}}}{\rho_1}$, which we shall in future denote by \mathfrak{A}_i ; hence it is the amplitude of the incident light proceeding from ∂F .

* Translated from Wiedemann's *Annalen*, 1878, No. 9.

The motion of the diffracted light is of the form

$$A \sin \left(2\pi \frac{t}{T} + \delta \right);$$

the general expression which contains its amplitude both for Fresnel's and Fraunhofer's phenomena is

$$A^2 = \mathfrak{N}_i^2 K^2 \left\{ \left(\iint \frac{\partial \rho_1}{\partial n} \cos p \partial \mathfrak{F} \right)^2 + \left(\iint \frac{\partial \rho_1}{\partial n} \sin p \partial F \right)^2 \right\}^*,$$

in which

$$p = \frac{2\pi}{\lambda} \left\{ \frac{x^2 + y^2 + z^2}{2} \left(\frac{1}{\rho_1} + \frac{1}{\rho_0} \right) - (x\alpha + y\beta + z\gamma) \right\};$$

and x, y, z are the coordinates of the element $\partial \mathfrak{F}$ of the imaginary surface which covers the aperture and possesses its boundary, n the normal to this element; and

$$\alpha = \cos \alpha^0 - \cos \alpha_1, \quad \beta = \cos \beta_0 - \cos \beta_1, \quad \gamma = \cos \gamma_0 - \cos \gamma_1,$$

if $\alpha_1 \beta_1 \gamma_1, \alpha_0 \beta_0 \gamma_0$ denote the direction-angles of the incident and the diffracted ray.

The equation of the surface \mathfrak{F} can in any case be put $z = \phi(x, y)$, from which we get $\frac{\partial \rho_1}{\partial n} = \cos(\rho_1 n)$ as a function

of x and y ; besides, $\partial \mathfrak{F}$ is $= \frac{\partial x \partial y}{\cos(nz)}$, the denominator of which can likewise be expressed by x and y ; so that a function of x and y only stands under the integral-symbol of the above expression, and the integration itself can be effected along x and y .

The amplitude A is entirely independent of the position of the coordinate-system; let us in future give it such a position that the incident ray, and therefore the $\alpha_1 \beta_1 \gamma_1$ direction, will fall very nearly in the plane of ZX ; by this the following considerations will be simplified.

Upon the carrying-out of the integrations in A the variables x and y vanish; their place is taken by the constants of the limitation of the aperture; and A depends on these and also on ρ_1 and $\rho_0, \alpha, \beta, \gamma$. Yet we have to do only with indefi-

* The factor $\frac{\partial \rho_1}{\partial n} = \cos(\rho_1 n)$ follows from the principle of the equality of the kinetic energies, as will be shown in a subsequent paper. W. Voigt also deduced it from Fresnel's theory (Wied. Ann. iv. p. 542 &c.), and likewise from the elasticity theory of light; it has, however, no influence at all upon the deduction or validity of the following proposition.

nitely small angles of diffraction; so that we can write:—

$$\alpha = (\alpha_1 - \alpha_0) \sin \alpha_0 = (\alpha_1 - \alpha_0) \sin \alpha_1,$$

$$\beta = (\beta_1 - \beta_0) \sin \beta_0 = (\beta_1 - \beta_0) \sin \beta_1,$$

$$\gamma = (\gamma_1 - \gamma_0) \sin \gamma_0 = (\gamma_1 - \gamma_0) \sin \gamma_1;$$

and

$$\cos^2 \alpha_0 + \cos^2 \beta_0 + \cos^2 \gamma_0 = 1,$$

$$\cos^2 \alpha_1 + \cos^2 \beta_1 + \cos^2 \gamma_1 = 1.$$

If in the penultimate equation we write

$$\alpha_0 = \alpha_1 + (\alpha_0 - \alpha_1), \quad \beta_0 = \beta_1 + (\beta_0 - \beta_1), \quad \gamma_0 = \gamma_1 + (\gamma_0 - \gamma_1),$$

and develop, we get

$$\cos \gamma_1 \sin \gamma_1 (\gamma_0 - \gamma_1) = -\cos \alpha_1 \sin \alpha_1 (\alpha_0 - \alpha_1) - \cos \beta_1 \sin \beta_1 (\beta_0 - \beta_1)$$

$$\text{or} \quad \gamma = -\alpha \frac{\cos \alpha_1}{\cos \gamma_1} - \beta \frac{\cos \beta_1}{\cos \gamma_1} = -\alpha \frac{\cos \alpha_0}{\cos \gamma_1} - \beta \frac{\cos \beta_0}{\cos \gamma_0}.$$

Consequently γ is a *linear* function of α and β ; hence, since according to our hypothesis ρ_0 and ρ_1 are constant, A depends only on α and β . We write this,

$$A^2 = \mathfrak{A}_i^2 K^2 \Phi(\alpha, \beta).$$

With the chosen position of the system of coordinates, the surface-elements that come into consideration are

$$\partial F = \rho_1^2 \partial \alpha_1 \partial \beta_1, \quad \partial f = \rho_0^2 \partial \alpha_0 \partial \beta_0;$$

from these we get

$$\mathfrak{A}_i = \left(\frac{\mathfrak{A}_0^2 \partial F}{\rho_1^2} \right)^{\frac{1}{2}} = (\mathfrak{A}_0^2 \partial \alpha_1 \partial \beta_1)^{\frac{1}{2}},$$

and the amplitude of the diffracted light is

$$A^2 = K^2 \mathfrak{A}_0^2 \partial \alpha_1 \partial \beta_1 \Phi(\alpha, \beta).$$

Therefore the *kinetic energy of the same light incident upon the element* ∂f ,

$$CA^2 \rho_0^2 \partial \alpha_0 \partial \beta_0 = CK^2 \mathfrak{A}_0^2 \rho_0^2 \partial \alpha_0 \partial \beta_0 \Phi(\alpha, \beta) \partial \alpha_1 \partial \beta_1. \quad (0)$$

Starting from this expression, we can further develop in two directions: the integration along $\alpha_0 \beta_0$ gives the energy incident upon the entire screen, which proceeds from ∂F ; the integration along $\alpha_1 \beta_1$ gives the energy from the whole of the luminous surface F , falling on the element ∂f . Let us carry out both operations.

1. $\alpha_0 \beta_0$ are the variables; then

$$\alpha = (\alpha_1 - \alpha_0) \sin \alpha_1, \quad \beta = \beta_1 - \beta_0,$$

because in the above-mentioned position of the system of co-ordinates β_1 and β_0 are very nearly $\frac{\pi}{2}$;

$$\partial\alpha_0 = -\frac{\partial\alpha}{\sin\alpha_1} = -\frac{\partial\alpha}{\cos\gamma_1}; \quad \partial\beta_0 = -\partial\beta;$$

$$\partial\alpha_0 \partial\beta_0 = \frac{\partial\alpha \partial\beta}{\cos\gamma_1}.$$

Hence we get for the kinetic energy of the diffracted light proceeding from ∂F and falling upon the entire screen:—

$$CK^2 \mathcal{N}_0^2 \rho_0^2 \partial\alpha_1 \partial\beta_1 \frac{1}{\cos\gamma_1} \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \Phi(\alpha, \beta) \partial\alpha \partial T. \quad (I.)$$

2. The variables are α_1, β_1 ; then

$$\alpha = (\alpha_1 - \alpha_0) \sin\alpha_0, \quad \beta = \beta_1 - \beta_0$$

(for the same reason as before),

$$\partial\alpha_1 = \frac{\partial\alpha}{\sin\alpha_0} = \frac{\partial\alpha}{\cos\gamma_0}, \quad \partial\beta_1 = \partial\beta,$$

$$\partial\alpha_1 \partial\beta_1 = \frac{\partial\alpha \partial\beta}{\cos\gamma_0}.$$

Therefore the kinetic energy of the diffracted light proceeding from the entire luminous surface FF , but falling only upon the element ∂f , is

$$CK^2 \mathcal{N}_0^2 \rho_0^2 \partial\alpha_0 \partial\beta_0 \frac{1}{\cos\gamma_0} \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \Phi(\alpha, \beta) \partial\alpha \partial\beta. \quad (II.)$$

The limits of the integration could be extended to $-\infty$ and $+\infty$, since indeed $\Phi(\alpha, \beta)$ remains finite only when the values of $\alpha_1 - \alpha_0, \beta_1 - \beta_0$ are very small, but otherwise vanishes.

Now, if we call the two elements ∂F and ∂f *conjugate* when their spherical angles in the aperture \mathfrak{F} are equal (consequently $\partial\alpha_0 \partial\beta_0 = \partial\alpha_1 \partial\beta_1$, and thus expressions I. and II. become identical),—expressed in words this leads to the following proposition:—

The kinetic energy of the diffracted light emanating from a large, uniformly luminous spherical surface FF , and falling on the element ∂f of the screen, is equal to the kinetic energy of the diffracted light proceeding from the conjugate luminous element ∂F , and falling on the entire spherical surface of the screen ff —provided that the line joining the conjugate elements goes

through the surface \mathfrak{F} of the aperture, or passes in its immediate vicinity, so that $\gamma_0 - \gamma_1$ is extremely small*.

We can at once avail ourselves of this result.

Let us provisionally assume only that the kinetic energies of the incident and diffracted light are equal; then, if we take into account only the light emanating from $\partial\mathfrak{F}$, out of expression I. comes the following equation:—

$$\begin{aligned} & C\mathfrak{A}_0^2 \partial\alpha_1 \partial\beta_1 \iint \frac{\partial\rho_1}{\partial n} \partial\mathfrak{F} \\ &= C\mathfrak{A}_0^2 \rho_0^2 \partial\alpha_1 \partial\beta_1 \frac{1}{\cos\gamma_1} \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \Phi(\alpha, \beta) \partial\alpha \partial\beta, \end{aligned}$$

from which

$$\int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \Phi(\alpha, \beta) \partial\alpha \partial\beta = \frac{\cos\gamma}{K^2 \rho_0^2} \iint \frac{\partial\rho_1}{\partial n} \partial\mathfrak{F}.$$

To find, on the other hand, the *intensity of the illumination* in the element ∂f produced by the entire surface $\mathfrak{F}\mathfrak{F}$, we have only to divide expression II. by $C\partial f = C\rho_0^2 \partial\alpha_0 \partial\beta_0$, and we get the square of this amplitude, denoted by A_∞ :—

$$A_\infty^2 = \frac{\mathfrak{A}_0^2 K^2}{\cos\gamma_0} \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \Phi(\alpha, \beta) \partial\alpha \partial\beta.$$

If we substitute in this equation the value of the double integral, and notice that γ_1 and γ_0 are *very nearly* equal, it becomes

$$A_\infty^2 = \frac{\mathfrak{A}_0^2}{\rho_0^2} \iint \frac{\partial\rho_1}{\partial n} \partial\mathfrak{F} = \frac{\mathfrak{F}_p}{\rho_0^2} \mathfrak{A}_0^2,$$

in which \mathfrak{F}_p signifies the other double integral.

This is a remarkable result, and expresses that *the illumination of the middle portion of the diffraction-image of a very large uniformly luminous spherical surface is directly proportional to the area \mathfrak{F}_p of the projection of the diffracting aperture upon the surface of the incident wave, but completely independent of the shape and position of the diffracting aperture.*

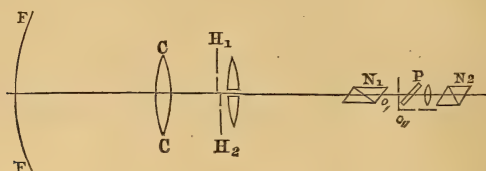
But we can with perfect justice invert this last train of thoughts and say:—If the proportionality to \mathfrak{F}_p of this illumination is established, then the presumption from which this proportionality resulted is correct; or, the principle of the

* I found this proposition at first for Fraunhofer's phenomena only; on my communicating it by letter to Professor Réthy of Klausenberg, he pointed out that the proposition can also be extended to Fresnel's phenomena.

equality of the energies is proved as soon as this principle is verified for *one* of these apertures.

The observations necessary for the purpose were carried out by the following method. In the vicinity of the focus of the collimator C C (fig. 2) was placed a portion of a spherical sur-

Fig. 2.



face F F made of fine paper oiled, the axis of symmetry of which fell into that of the collimator; behind F F was a petroleum-lamp burning with a large steady flame, which very equally illuminated the surface F F (the slight differences of intensity at its margin have no influence on the observation, since for these $\Phi(\alpha, \beta)$ already vanishes). The apparent magnitude of F F for the middle of the collimator amounted to about 8° ; therefore its dimensions were quite sufficient.

In front of one of the half-lenses, H_1 , of a heliometer a square aperture was brought, and remained there during the entire observation; before the other half-lens, H_2 , plane- and space-apertures were consecutively fixed, of the most varied shapes, as well as consisting some of one and others of a plurality of parts, and their dimensions and position were accurately determined. Both the half-lenses remained fixed during the observation; and their axes coincided exactly with that of C C.

Before the focus of the heliometer the rotating Nicol, N_1 , polarized the diffracted light; and the circular diaphragm o , placed in the focus of the ocular, permitted only the middle part of the diffraction image to be observed, while the parallel-plane glass plate P, inclined 45° to the axis of the tube, reflected the image of the small circular aperture o , exactly in such a way that o , and the image of o , in the ocular stood in immediate juxtaposition. The second Nicol, N_2 , placed before the ocular, retained its position unaltered during the observation.

In the observation properly so called, one half-lens of the heliometer was covered and the other left uncovered; and now N_1 was rotated until the images of o , and o , visible in the ocular both possessed the same intensity; the former half-lens

was now uncovered, and the other covered, and by repeated rotation of N_1 the equality of intensity of o_1 and o_{11} restored. These two positions of N_1 gave the relative intensity of the illumination of o_1 in the first and in the second case.

Now, as the equality of the kinetic energies for rectangular apertures had, in a previous investigation*, been demonstrated as actually existing, the present investigation needed only to be directed to the proportionality of the illumination in o_1 to \mathfrak{F}_p .

There was found here, as after the above examination was already highly probable, an exact agreement, *within the limits of errors of observation*, between the theoretic conclusions and the results of observation.

On the ground of these investigations we are justified in pronouncing it a proposition confirmed by experiment, that, *when the diffraction-angles are small, whatever the shape of the aperture, the kinetic energy of the incident light is equal to the kinetic energy of the diffracted light.*

Phys. Inst. Univ. Budapest,
June 15, 1878.

VII. *On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules.* By WILLIAM CROOKES, F.R.S., V.P.C.S.†

INDUCTION Spark through Rarefied Gases.—*Dark Space round the Negative Pole.*—The author has examined the dark space which appears round the negative pole of an ordinary vacuum-tube when the spark from an induction-coil is passed through it. He describes many experiments with different kinds of poles, a varying intensity of spark, and different gases, and arrives at the following propositions:—

Illumination of Lines of Molecular Pressure.—*a.* Setting up an intense molecular vibration in a disk of metal by electrical means excites a molecular disturbance which affects the surface of the disk and the surrounding gas. With a dense gas, the disturbance extends a short distance only from the metal; but as rarefaction continues, the layer of molecular disturbance increases in thickness. In air at a pressure of 0.078 millim. this molecular disturbance extends for at least 8 millims. from the surface of the disk, forming an oblate spheroid around it.

b. The diameter of this dark space varies with the exhaustion, with the kind of gas in which it is produced, with the temperature of the negative pole, and, in a slight degree, with the intensity of the spark. For equal degrees of exhaustion it is greatest in hydrogen and least in carbonic acid, as compared with air.

* Wiedemann's *Annalen*, vol. iii. p. 568.

† Abstract of a paper read before the Royal Society, Dec. 5, 1878.

c. The shape and size of this dark space do not vary with the distance separating the poles, nor (or only very slightly) with alteration of battery-power, nor with intensity of spark. When the power is great the brilliancy of the unoccupied parts of the tube overpowers the dark space, rendering it difficult of observation; but, on careful scrutiny, it may still be seen unchanged in size; nor does it alter even when, with a very faint spark, it is scarcely visible. On still further reduction of the power it fades entirely away, but without change of form.

The author describes numerous experiments, devised to ascertain if this visible layer of molecular disturbance is identical with the invisible layer of molecular pressure or stress, the investigation of which has occupied him for some years.

The Electrical Radiometer.—One of these experiments is as follows:—An ordinary radiometer is made, with aluminium disks for vanes, each disk coated with a film of mica. The fly is supported by a hard steel cup instead of a glass cup; and the needle-point on which it works is connected by means of a wire with a platinum terminal sealed into the glass; at the top of the radiometer-bulb a second terminal is sealed in. The radiometer can therefore be connected with an induction-coil, the movable fly being made the negative pole.

Passing over the phenomena observed at low exhaustions, the author finds that, when connected with the coil, a halo of a velvety violet light forms on the metallic side of the vanes, the mica side remaining dark throughout these experiments. As the pressure diminishes, a dark space is seen to separate the violet halo from the metal. At a pressure of half a millim. this dark space extends to the glass, and positive rotation commences.

On continuing the exhaustion, the dark space further widens out and appears to flatten itself against the glass, and the rotation becomes very rapid.

When aluminium cups are used for the vanes instead of disks backed with mica, similar appearances are seen. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. On further exhaustion, the dark space on the convex side touches the glass, when positive rotation commences, becoming very rapid as the dark space further increases in size and ultimately flattens against the glass.

Convergence of Molecular Rays to a Focus.—The subject next investigated is the convergence of the lines of force to a focus, as observed with the aluminium cup. As this could not be accomplished during rapid rotation, an instrument was made having the cup-shaped negative pole fixed instead of movable. On exhaustion, the convergence of the lines of force to a focus at the concave side was well observed. When the dark space is very much larger than the cup, it forms an irregular ellipsoid, drawn in towards the focal

point. Inside the luminous boundary a focus of dark violet light can be seen converging, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space—the whole appearance being strikingly similar to the rays of the sun reflected from a concave mirror through a foggy atmosphere.

Green Phosphorescent Light of Molecular Impact.—At very high exhaustions the dark space becomes so large that it fills the tube. Careful scrutiny still shows the presence of the dark violet focus; and the part of the glass on which fall the rays diverging from this focus shows a sharply defined spot of greenish-yellow light. On still further exhaustion, and especially if the cup is made positive, the whole bulb becomes beautifully illuminated with greenish-yellow phosphorescent light.

This greenish-yellow phosphorescence, characteristic of high exhaustions, is frequently spoken of in the paper. It must be remembered, however, that the particular colour is due to the special kind of soft German glass used. Other kinds of glass phosphoresce in a different colour. The phosphorescence takes place only under the influence of the negative pole. At an exhaustion of 4M* no light other than this is seen in the apparatus. At 0.9M the phosphorescence is about at its maximum. When the exhaustion reaches 0.15M, the spark has a difficulty in passing, and the green light appears occasionally in flashes only. At 0.06M the vacuum is almost non-conductive; and a spark can be forced through only by increasing the intensity of the coil and well insulating the tube and wires leading to it. Beyond that exhaustion nothing has been observed.

Focus of Molecular Force.—In an apparatus specially constructed for observing the position of the focus the author found that the focal point of the green phosphorescent light was at the centre of curvature, showing that the molecules by which it is produced are projected in a direction normal to the surface of the pole. Before reaching the best exhaustion for the green light, another focus of blue-violet light is observed; this varies in position, getting further from the pole as the exhaustion increases. In the apparatus described, at an exhaustion of 19.3M, these two foci are seen simultaneously, the green being at the centre of curvature, while the blue focus is at nearly twice the distance.

Nature of the Green Phosphorescent Light.—The author adduces the following characteristics of the green phosphorescent light, as distinguishing it from the ordinary light observed in vacuum-tubes at lower exhaustions:—

a. The green focus cannot be seen in the space of the tube, but where the projected beam strikes the glass only.

b. The position of the positive pole in the tube makes scarcely any difference to the direction and intensity of the lines of force which produce the green light. The positive pole may be placed in the tube either at the extremity opposite the negative pole, or below it, or by its side.

* M signifies the millionths of an atmosphere.

c. The spectrum of the green light is a continuous one, most of the red and the higher blue rays being absent ; while the spectrum of the light observed in the tube at lower exhaustions is characteristic of the residual gas. No difference can be detected by spectrum-examination in the green light, whether the residual gas be nitrogen, hydrogen, or carbonic acid.

d. The green phosphorescence commences at a different exhaustion in different gases.

e. The viscosity of a gas is almost as persistent a characteristic of its individuality as its spectrum. The author refers to a preliminary note and a diagram* of the variation of viscosity of air, hydrogen, and other gases at exhaustions between 240 M and 0.1 M. From these and other unpublished results, the author finds that the viscosity of a gas undergoes very little diminution between atmospheric pressure and an exhaustion at which the green phosphorescence can be detected. When, however, the spectral and other characteristics of the gas begin to disappear, the viscosity also commences to decline ; and at an exhaustion at which the green phosphorescence is most brilliant the viscosity has rapidly sunk to an insignificant amount.

f. The rays exciting green phosphorescence will not turn a corner in the slightest degree, but radiate from the negative pole in straight lines, casting strong and sharply defined shadows from objects which happen to be in their path. On the other hand, the ordinary luminescence of vacuum-tubes will travel hither and thither along any number of curves and angles.

Projection of Molecular Shadows.—The author next examines the phenomena of shadows cast by the green light. The best and sharpest shadows are cast by flat disks and not by narrow-pointed poles ; no green light whatever is seen in the shadow itself, no matter how thin, or whatever may be the substance from which it is thrown.

From these and other experiments, fully described in the paper, he ventures to advance the theory that the induction-spark actually illuminates the lines of molecular pressure caused by the electrical excitement of the negative pole. The thickness of the dark space is the measure of the mean length of the path between successive collisions of the molecules. The extra velocity with which the molecules rebound from the excited negative pole keep back the more slowly moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the collisions.

When the exhaustion is sufficiently high for the mean length of path between successive collisions to be greater than the distance between the fly and the glass, the swiftly moving rebounding molecules spend their force, in part or in whole, on the sides of the vessel, and the production of light is the consequence of this sudden arrest of velocity. The light actually proceeds from the glass, and

* Proc. Roy. Soc. Nov. 16, 1876, vol. xxv. p. 305.

is caused by fluorescence or phosphorescence on its surface. No light is produced by a mica or quartz screen; and the more fluorescent the material the better the luminosity. Here the consideration arises that the greenish-yellow light is an effect of the direct impact of the molecules in the same electrical state on the surface of the glass. The shadows are not optical, but are molecular shadows, revealed only by an ordinary illuminating effect; this is proved by the sharpness of the shadow when projected from a wide pole.

Phosphorescence of Thin Films.—An experiment is next described in which a film of uranium glass, sufficiently thin to show colours of thin plates, is placed in front of a thick plate of the same glass, the whole being enclosed in a tube with terminals and exhausted to a few millionths of an atmosphere. Of this the following observations are recorded:—

a. The uranium film, being next to the negative pole, casts a strong shadow on the plate.

b. On making contact with the coil, the thin film flashes out suddenly all over its surface with a yellowish phosphorescence, which, however, instantly disappears. The uncovered part of the plate does not become phosphorescent quite suddenly, but the phosphorescence is permanent as long as the coil is kept at work.

c. With an exceedingly faint spark the film remains more luminous than the plate; but on intensifying the spark, the luminosity of the film sinks, and that of the uncovered part of the plate increases.

d. If a single intense spark be suddenly sent through the tube, the film becomes very luminous, while the plate remains dark.

These experiments are conclusive against the phosphorescence being an effect of the radiation of the phosphorogenic ultra-violet light from a thin layer of arrested molecules at the surface of the glass; for were this the case, the film could under no circumstances be superior to the plate.

The momentary phosphorescence and rapid fading of the film prove more than this. The molecular bombardment is too much for the thin film. It responds thereto at first, but immediately gets heated by the impacts, and then ceases to be luminous. The plate, however, being thick, bears the hammering without growing hot enough to lose its power of phosphorescing.

Mechanical Action of Projected Molecules.—When the coil was first turned on, the thin film was driven back at the moment of becoming phosphorescent, showing that an actual material blow had been given by the molecules. Experiments are next described in which this mechanical action is rendered more evident. A small rotating fly, capable of being moved about in any part of an exhausted bulb, is used as an indicator; and by appropriate means the molecular shadow of an aluminium plate is projected along the bulb. Whether entirely in or entirely out of the shadow, the indicator scarcely moves; but when immersed so that one half is exposed to molecular impact, the fly rotates with extreme velocity.

Magnetic Deflection of Lines of Molecular Force.—With this apparatus another phenomenon was investigated. It is found that the stream of molecules whose impact on the glass occasions evolution of light is very sensitive to magnetic influence; and by bringing one pole of an electromagnet, or even of a small permanent magnet, near, the shadow can be twisted to the right or to the left.

When the little indicator was placed entirely within the molecular shadow no movement was produced. As soon, however, as an adjacent electromagnet was excited, the shadow was twisted half off the indicator, which immediately rotated with great speed.

The Trajectory of Molecules.—The amount of deflection of the stream of molecules forming a shadow is in proportion to the magnetic power employed.

The trajectory of the molecules forming the shadow is curved when under the magnetic influence: the action of the magnet is to twist the trajectory of the molecules round in a direction at an angle to their free path, and to a greater extent as they are nearer the magnet, the direction of twist being that of the electric current passing round the electromagnet.

Laws of Magnetic Deflection.—An apparatus was constructed so that the deflection of a spot of light was used instead of that of a shadow, a horseshoe magnet being placed underneath the negative pole to deflect the trajectory. The action of the north pole being to give the line of molecules a spiral twist one way, and that of the south pole being to twist it the other way, the two poles side by side compel the line to move in a straight line up or down, along a plane at right angles to the plane of the magnet and a line joining its poles.

The ray of molecules does not appear to obey Ampère's law, as it would were it a perfectly flexible conductor, joining the negative and the positive pole. The molecules are projected from the negative; but the position of the positive pole—whether in front, at the side, or even behind the negative pole—has no influence on their subsequent behaviour, either in producing phosphorescence, or mechanical effects, or in their magnetic deflection. The magnet gives their line of path a spiral twist, greater or less according to its power, but diminishing as the molecules get further off.

Numerous experiments were tried in this apparatus with different gases, and with the magnet in and out of position.

Working with exhausted air it was found that the spot of green phosphorescence on the screen is visible at an exhaustion of 102.6 millim., when the mean free path of the molecules, measured by the thickness of the dark space round the negative pole, is only 12 millims. Hence it follows that a number of molecules sufficient to excite green phosphorescence on the screen are projected the whole distance from the pole to the screen, or 102 millims., without being stopped by collisions.

Alteration of Molecular Velocity.—If we suppose the magnet to be permanently in position, and thus to exert a uniform downward pull on the molecules, we perceive that their trajectory is much

curved at low exhaustions, and gets flatter as the exhaustion increases. A flatter trajectory corresponds to a higher velocity. This may arise from one of two conditions: either the initial impulse given by the negative pole is stronger, or the resisting medium is rarer. The latter is probably the true one. The molecules which produce the green phosphorescence must be looked upon as in a state differing from those arrested by frequent collisions. The latter impede the velocity of the free molecules and allow longer time for magnetism to act on them; for, although the deflecting force of magnetism might be expected to increase with the velocity of the molecules, Prof. Stokes has pointed out that it would have to increase as the square of the velocity, in order that the deflection should be as great at high as at low velocities.

Comparing the free molecules to cannon-balls, the magnetic pull to the earth's gravitation, and the electrical excitation of the negative pole to the explosion of the powder in the gun, the trajectory will be flat when no gravitation acts, and curved when under the influence of gravitation. It is also much curved when the ball passes through a dense resisting medium; it is less curved when the resisting medium gets rarer; and, as already shown, intensifying the induction spark, equivalent to increasing the charge of powder, gives greater initial velocity, and therefore flattens the trajectory. The parallelism is still closer if we compare the evolution of light seen when the shot strikes the target, with the phosphorescence on the glass screen from molecular impacts.

Focus of Heat of Molecular Impact.—The author finally describes an apparatus in which he shows that great heat is evolved when the concentrated focus of rays from a nearly hemispherical aluminium cup is deflected sideways, to the walls of the glass tube, by a magnet. By using a somewhat larger hemisphere, and allowing the negative focus to fall on a strip of platinum-foil, the heat rises to the melting-point of platinum.

An Ultra-gaseous State of Matter.—The paper concludes with some theoretical speculations on the state in which the matter exists in these highly-exhausted vessels. The modern idea of the gaseous state is based upon the supposition that a given space contains millions of millions of molecules in rapid movement in all directions, each having millions of encounters in a second. In such a case, the length of the mean free path of the molecules is exceedingly small as compared with the dimensions of the vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed. But by great rarefaction the free path is made so long that the hits in a given time may be disregarded in comparison with the misses, in which case the average molecule is allowed to obey its own motions or laws without interference; and if the mean free path is comparable to the dimensions of the vessel, the properties which constitute gasity are reduced to a minimum, and the matter becomes exalted to an ultra-gaseous state, in which the very decided but

hitherto masked properties now under investigation come into play.

Rays of Molecular Light.—In speaking of a ray of molecular light the author has been guided more by a desire for conciseness of expression than by a wish to advance a novel theory. But he believes that the comparison, under these special circumstances, is strictly correct, and that he is as well entitled to speak of a ray of molecular or emissive light when its presence is detected only by the light evolved when it falls on a suitable screen, as he is to speak of a sunbeam in a darkened room as a ray of vibratory or ordinary light when its presence is to be seen only by interposing an opaque body in its path. In each case the invisible line of force is spoken of as a ray of light; and if custom has sanctioned this as applied to the undulatory theory, it cannot be wrong to apply the expression to emissive light. The term emissive light must, however, be restricted to the rays between the negative pole and the luminous screen; the light by which the eye then sees the screen is, of course, undulatory.

The phenomena in these exhausted tubes reveal to physical science a new world—a world where matter exists in a fourth state, where the corpuscular theory of light holds good, and where light does not always move in a straight line—but where we can never enter, and in which we must be content to observe and experiment from the outside.

VIII. Notices respecting New Books.

- I. *Report on the Administration of the Meteorological Department of the Government of India in 1876-77.*
- II. *Report on the Meteorology of India in 1876.* By HENRY F. BLANFORD, *Meteorological Reporter to the Government of India. Second Year.* Calcutta, 1878.
- III. *Indian Meteorological Memoirs.* Published by order of His Excellency the Viceroy and Governor General of India in Council, under the Direction of HENRY F. BLANFORD, *Meteorological Reporter to the Government of India.* Calcutta, 1878.

THESE publications make us acquainted with the progress of meteorological work in India during the year 1876, under the able superintendence of Mr. Blanford. From them we learn that the system of Meteorological Observation, the administration of which was concentrated by order of the Government in a single central office, has not only worked well, but that the area over which it now extends embraces $43^{\circ} 30'$ of longitude and 24° of latitude; viz. from 51° to $94^{\circ} 30'$ of east longitude, and from 10° to 34° of north latitude.

For many years the pursuit of meteorology consisted in observing the indications of the instruments at certain periods of the day, and deducing from the readings thus obtained the mean temperature or pressure at these periods; but of late years a much

more scientific method has obtained. Excellent as many of these observations were, furnishing, as in Howard's case, the elements of climate, they were still isolated; for while they recorded weather-changes, it was only at the particular localities where they were observed. For real advancement a network of stations was requisite: to organize such a network was clearly out of the power of isolated observers; only large Associations could undertake a work of the kind; and even under the auspices of an old-established association, such an undertaking might lack the necessary stability in order to carry it out to a useful end. Governments alone could really grapple with such an extensive subject as is presented to us in meteorology; and this has been accomplished by both the American and Indian Governments. In India many important questions, bearing in no small degree on the welfare and even the lives of the inhabitants, have arisen in consequence of the widespread calamities with which the peninsula has been visited, particularly the recent famines. To such questions Mr. Blanford, the Meteorological Reporter to the Government of India, has directed his most sedulous attention, and has sought to elucidate the links of the chain of causation which led to and culminated in the famine of 1876. Two of these links have been ascertained—one to consist of the failure of rainfall in the western and southern provinces, where the staple vegetation was withered to the condition of hay under an ever cloudless sky, the other of a superabundant outpour over the Burmese peninsula and the Bay of Bengal of the rain withheld from the Provinces, which overcharged the Irawadi and caused those disastrous floods that washed away and drowned the rice crops: thus the famine was brought about by a failure and an excess of rainfall—the failure being largely attributable to the prevalence of northerly and north-westerly land winds, and the excess to a vapour-laden current from the south-west, which, recurving cyclonically around the Bay of Bengal, discharged its burden over the Bay and on the south-east coast of the peninsula.

This second report of the Department of Indian Meteorology is an admirable specimen of the work effected by the Indian Government.

IX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. vi. p. 313.]

November 20, 1878.—R. Etheridge, Esq., F.R.S.,
Vice-President, in the Chair.

THE following communications were read:—

1. "On the Upper-Greensand Coral Fauna of Haldon, Devonshire." By Prof. P. Martin Duncan, M.B. Lond., F.R.S., F.G.S., &c.

Phil. Mag. S. 5. Vol. 7. No. 40. Jan. 1879.

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2. "Notes on *Pleurodon affinis*, sp. ined., Agassiz, and Description of three spines of Cestracionts from the Lower Coal-measures." By J. W. Davis, Esq., F.G.S.

3. "On the Distribution of Boulders by other Agencies than that of Icebergs." By C. E. Austin, Esq., C.E., F.G.S.

The author is of opinion that if boulders had been distributed by floating ice they should now be accumulated "in certain parallel and quasi-concentric lines." This he does not find to be the case in Sweden and North Russia, where the distribution is irregular. From the mode of distribution he infers that the boulders must have been uniformly distributed in the ice blocks, whereas they ought to have been most abundant at the base. The boulders are not connected with existing ravines, and the author does not see how ice can move on a plain. He has never seen a glacier-moraine, but thinks that these boulders are not like the blocks in moraines. In Siberia, Portugal, and Rio Janeiro the author has seen solid nuclei in decomposed granite. Such boulders, as he notices, may be left after decomposition of a once flowing molten mass, of which they have formed a part or in which they have been entangled.

December 4.—Henry Clifton Sorby, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. "On some Mica-Traps from the Kendal and Sedbergh Districts." By Prof. T. G. Bonney, M.A., F.R.S., F.G.S., and F. T. S. Houghton, Esq., B.A.

The rocks described by the authors are mapped by the Geological Survey on quarter sheets 98 N.E., 98 S.E., and 97 N.W., and in parts briefly mentioned in the accompanying memoirs, under the generic name mica-trap. Seventeen examples are described macroscopically and microscopically; and of eight, chemical analyses are given. It appears better to call one a porphyrite and two diorites (micaceous varieties). The remainder are all characterized by abundance of mica (biotite). Augite also appears to have been generally a constituent; but it has almost invariably been replaced by secondary products—calcite, dolomite, viridite, &c. Three are crystalline in structure; one of these is named minette, the others kersantite. The remaining eleven show a microcrystalline or cryptocrystalline base. It is proposed to call eight of them minette-felsite, the rest kersantite-porphyrite. These rocks commonly occur in rather narrow dykes; they are intrusive in Silurian strata, and, in the authors' opinion, are undoubtedly true igneous rocks.

2. "Pleistocene Notes on the Cornish Coast near Padstow." By W. A. E. Ussher, Esq., F.G.S.

In this paper the author described certain deposits seen in a small bay near St. Enodock's chapel, and known as Daymer Bay, and in section at Greenway cliffs. The former included a portion of raised beach, and a reef of consolidated old beach and a peaty deposit below

high-water mark, the raised beach indicating a depression of from 5 to 10 feet and a subsequent elevation of more than that amount, during a pause in which the lower beach was formed. The further elevation of the coast was sufficient to favour the growth of forests furnishing the peaty bed, which a subsequent subsidence has brought down to its present level. Greenway cliffs consist of grey slates, resting against which, in two places, are old consolidated blown sands; about 5 feet above high-water mark is a raised beach, near which the face of the cliff consists of "head" capped by gravel. The author discussed the relative ages of these deposits, and inclined to regard the gravel as a fluviatile deposit, and the stony loam or "head" as an ancient talus or flood-gravel, both deposited before the raised beach.

3. *The Pleistocene History of Cornwall.* By W. A. E. Ussher, Esq., F.G.S.

In the first part of this paper the author, from his own observations and the writings of other geologists, gave detailed descriptions of the various superficial deposits of Cornwall as exposed in numerous coast-sections.

In the second part he discussed the relative ages of these deposits, for which he proposed the following classification:—

1. The oldest beds described are patches of quartzose gravel, found up to 400 feet above the present sea-level; these are regarded by the author as of fluviatile origin, and as being possibly redeposited Tertiary beds. Their age may be any thing between Cretaceous and Glacial.

2. Boulder-gravels, from 40 to 50 feet above sea-level.

3. Raised beaches, up to 15 feet above sea-level.

4. Old blown sand closely associated with the raised beaches.

5. "Head" or talus of angular fragments lying upon the raised beaches, and therefore of younger date than the latter.

6. Stream-tin gravels, evidently older than the forest stratum.

7. Submerged forests, evidently occupying a long period subsequent to the deposition of the stream-tin gravels.

8. Recent marine and fluviatile deposits.

In conclusion he remarked on the paucity of superficial deposits in Cornwall, the absence of evidence of glacial conditions, and the proofs of great changes in the level of the area.

X. Intelligence and Miscellaneous Articles.

ON THE FIGURE OF THE PLANET MARS.

LETTER FROM PROFESSOR H. HENNESSY.

M. AMIGUES published in 1874, in the *Comptes Rendus* of the Academy (vol. lxxviii. p. 1556), a memorandum on the Configuration of the planet Mars, which seems to me to verify completely some results at which I arrived some time ago. The author says:—

"I propose in this memorandum to remove this objection [namely, the objection to the hypothesis of the original fluidity of the planets on the ground of the exceptional magnitude of the oblateness of the planet Mars] by showing that geometers have not handled the problem of spheroids in such a general way as could be wished." And after having indicated the method he employs, he says:—"This calculation, made by the usual methods (that is to say, by employing Laplace's functions and neglecting quantities of the second order), leads me to the results which follow."

Relatively to these points, I may be permitted to remark that I have long ago investigated the same problem of spheroidal attractions and by precisely the same methods—that is, by the application of Laplace's functions. At first I applied the result of my calculations to the question of the figure of the Earth, with the aim of thoroughly studying the theory which endeavours to explain its spheroidal form by the attrition of its surface. This theory was at first proposed by Playfair in his 'Illustrations of the Huttonian* System;' and it has been put forward afresh by Sir John Herschel in his 'Outlines of Astronomy.' It also acquires some interest because it has been cited by Sir Charles Lyell and serves as a foundation for the opinion which he maintains in his 'Principles of Geology,' as to the earth's figure.

The results which I have obtained show that this theory cannot be upheld; for the greatest ellipticity which the earth could possess from the action of attrition cannot exceed $\frac{1}{404}$, a fraction which differs considerably from that which is usually admitted as the result of observations.

In 1864 I had for the first time applied my calculations to the question of Mars, in a communication to the British Association; and a short extract of my paper was published.

In February 1870 I published a memoir in 'Atlantis' (No. ix., 8vo, London, February 1870) on the Configuration of the Planet Mars; and I applied to Mars the mathematical results of my previous investigations. I found (p. 178) an equation giving the ellipticity as a function of the mean density D' and of the surface-density D of the planet,

$$\epsilon = \frac{5q}{10 - 6 \frac{D'}{D}} = \frac{q}{2 \left(1 - \frac{3}{5} \frac{D'}{D} \right)}.$$

In the equation q is the ratio of the centrifugal force to gravity.

Now, if we employ the notation of M. Amigues, q will be replaced by ϕ , and D' by ρ' , and D by ρ , which gives

$$\epsilon = \frac{5\phi}{10 - 6 \frac{\rho'}{\rho}} = \frac{\phi}{2 \left(1 - \frac{3}{5} \frac{\rho'}{\rho} \right)},$$

a formula which is precisely that given by M. Amigues.

* In the *Comptes Rendus* "Hutton" is misprinted "Newton."

I have also deduced from my formula this conclusion—that if the greatest oblateness sometimes attributed to Mars be admitted, we must conclude that its surface-density is greater than the interior density of the planet; but as such a conclusion seems to me to be contrary to the laws of Physics, if the constitution of Mars be like that of the Earth, until more complete observations shall have been made, I prefer accepting the conclusions of Bessel, Johnson, Oudemans, and Winnecke, who admit an almost insensible oblateness for Mars.

An extract from my previous researches on the theory of the form of the Earth as a result of attrition appeared in several scientific journals some years ago; I am sure, however, that the results got by M. Amigues were obtained in a manner altogether independent of, and without his having had any knowledge of my investigations.

The entire agreement of his calculations with those I had previously made is not only interesting as far as regards Mars, but they confirm the idea I had formerly upheld in opposition to the theory of Playfair, Herschel, and Lyell, on the form and structure of the Earth.—*Comptes Rendus de l'Académie des Sciences*, No. 17, Oct. 22, 1878.

ON A NEW PHENOMENON OF STATIC ELECTRICITY.

BY E. DUTER.

I have the honour of submitting to the Academy the description of an experiment which proves that, in certain cases, electrization changes the volume of bodies.

In order to make the experiment, we procure a large thermometer-case. With this we make a condenser of which it is the insulator by passing into its interior a platinum wire, filling it with water, and pasting to its outer surface a sheet of tinfoil. We have thus a Leyden jar, which we charge by the usual methods. As soon as it receives the charge the surface of the water is seen to sink, remain stationary as long as the charge continues, and instantly resumes its former level with the discharge. As in a condenser the electricity resides only in the insulator, it is natural to conclude from this experiment that the glass dilates. We obtain a first confirmation of this idea on remarking that, whatever may be the nature of the armatures—tinfoil, water, saline solutions, or mercury—the same apparent contraction of the liquid inside is observed. To remove all doubt, I modified the apparatus by putting the Leyden jar into a closed glass case, terminated also with a thermometric stem, and likewise filled with a conducting liquid. In this arrangement the liquid of the inner reservoir forms the internal armature of the condenser, the liquid in the case forms the external armature, and the surface of the inner glass is the insulator. It is this which, if our provisions are correct, should be enlarged by the electrization. We find, in fact, that the water descends in the thermometric tube of the inner vessel, and a sensibly equal quantity ascends in the measuring-tube of the outer case. As soon as the

apparatus is discharged, every thing returns to the initial state: the liquid which had descended in the tube of the inner vessel rises again; and that which had risen in the tube of the outer case re-descends. We must therefore conclude that the internal capacity and the external volume increase during the charge of a Leyden jar.

To leave no doubt on the subject, I will review the objections which can be made to this conclusion.

(1) The effect cannot be attributed to a rise of temperature, since the discharge causes it to disappear immediately instead of increasing it.

(2) Electric pressure might be suggested as the cause; but that would be the same on both faces of the dielectric, and then it would produce a diminution of volume instead of the increase observed.

(3) It might also be said that the liquid does not perfectly wet the glass before the electrization, and that afterwards, in consequence of attraction, more intimate contact is produced, giving rise to an apparent contraction of the liquid. But then the same phenomenon ought to be produced for the exterior liquid—which does not take place.

(4) Again, different properties of the positive and negative armatures might be mentioned. But if the communications of the apparatus with the electrical machine be reversed, the direction of the phenomenon does not change.

In short, it is established that, in a Leyden jar, the insulator undergoes, through the electrization, a dilatation which can neither be accounted for by a rise of temperature nor by an electric pressure. We therefore find ourselves in the presence of a new phenomenon; as to the interpretation that may be given of it, although several present themselves to the mind, it would be premature to discuss them.

M. Jamin, in presenting the above Note to the Academy, was anxious to acknowledge that, ten years since, M. Govi had made, and published in the *Actes de l'Académie de Turin*, the first part of the experiments of M. Duter. M. Govi observed that the internal volume seems to increase during the charging of a Leyden jar; and he attributed this effect to a contraction of the liquid which it contains; but he did not institute any experiment to show that the external volume is augmented. This is what M. Duter has done; and it has led him to a conclusion contrary to that of M. Govi—namely, that the effect observed is simply due to a dilatation of the dielectric case.—*Comptes Rendus de l'Académie des Sciences*, Nov. 23, 1878, tome lxxxvii. pp. 828–830.

NEW OBSERVATIONS ON THE PART PLAYED BY PRESSURE IN
CHEMICAL PHENOMENA. BY M. BERTHELOT.

Permit me to call attention to a circumstance in the remarkable experiments of M. Pictet on the liquefaction of oxygen and hydrogen. Perhaps it will not be uninteresting to remark that the

decomposition of chlorate of potassium into oxygen and chloride of potassium, *an exothermic reaction, and not limited by its inverse*, is not arrested by a pressure of 320 atmospheres. In fact, from my measurements, the reaction



would liberate at the ordinary temperature $+11.0$. At about 400° , the chlorate being fused and the chloride solid, the amount liberated could not but be augmented.

It is the same with the decomposition of formate by hydrate of potass, the hydrogen continuing to be liberated even under a pressure of more than 600 atmospheres. Here, again, is an exothermic reaction not limited by its inverse. In fact the transformation of the system $\text{C}_2\text{HKO}_4 + \text{KHO}_2$ into $\text{C}_2\text{O}_6\text{K}_2 + \text{H}_2$ would liberate at the ordinary temperature

$$277.8 - 259.4 = 18.4 \text{ calories.}$$

At about 400° – 500° , all the substances being supposed to be fused, the heat disengaged would not be much modified; for the heats of fusion of the known salts but little exceed $+4$ calories, and the initial system comprises 2 fused molecules, the final system containing only 1.

Thus the exothermic reactions persist, whatever may be the amount of the pressure. It is nevertheless probable that the velocity of such a reaction is changed, and perhaps also the temperature at which it is effected; but the reaction itself does not cease to take place. This is a fresh proof in support of the opinions enunciated by the author of the present note, on a question so important to mechanical chemistry—opinions contested at first, but which derive fresh support from every new observation*.—*Annales de Chimie et de Physique*, October 1878, tome xv. p. 149.

ON AN AUTOMATIC CURRENT-REGULATOR. BY M. HOSPITALIER.

The apparatus which we have the honour to present to the Academy is composed of a resistance-coil wound in one layer only, the wire of which has been denuded along a generatrix of the spiral over a width of about 1 centimetre. A lever, somewhat convex, and forming a divider, is applied to the denuded part of the wire. This divider is attached at one of its extremities to an armature placed before an electro-magnet, in which the current circulates which is to be regulated. An antagonizing spring supports the lever at its other extremity. The circuit is formed by the resistance-coil, the lever, and the electromagnet. The apparatus being regulated for a determined intensity, the divider introduces into the circuit a certain number of turns of the coil. If the current increases in intensity, the electromagnet attracts its armature more strongly, the divider shifts its fulcrum and introduces into the circuit a greater number

* See *Chimie organique fondée sur la Synthèse*, t. ii. p. 349 (1860); *Annales de Chimie et de Physique*, 3^e sér. t. lxvi. pp. 41 & 59, t. lxviii. p. 239, and especially 4^e série, t. xviii. p. 95, and 5^e série, t. xii. p. 310, &c.

of turns of the coil ; the resistance increases, and the intensity is diminished. The opposite effect is produced if the intensity of the current diminishes.

By suitably regulating the force of the antagonizing spring, the electromagnet, the distribution of the wire upon the bobbin, and the curvature of the divider, we can render the system astatic ; and then the apparatus gives a mathematically constant current.

In practice we can maintain the intensity of the current between two limits fixed beforehand, and as close as we will.

From an industrial point of view, the apparatus can be applied to electrotyping, to the incandescence of wires of platinum or iridium (to prevent their fusion), and, if the problem should one day receive its practical solution, to the distribution of electricity in dwelling-houses, where the apparatus will play the part of a real meter and divider of the electric current.—*Comptes Rendus de l'Académie des Sciences*, Dec. 9, 1878, t. lxxxvii. p. 920.

ON THE PHYSICAL STATE OF CENTRAL EUROPE IN THE TERTIARY PERIOD (AS DISPLAYED IN THE WRITINGS OF PROF. O. HEER).

BY M. VAN TIEGHEM.

By publishing, in 1828, his *Histoire des Végétaux fossiles*, M. Ad. Brongniart laid the foundation of vegetable palæontology. Having a few years afterwards, about 1835, entered upon the new path, M. Oswald Heer was not long in gaining therein one of the highest ranks, which he has been able to keep.

The study of the plants, and also of the insects, of the Tertiary formations had till then been much neglected ; and it was to these that Prof. Heer almost exclusively devoted the fine series of investigations which he indefatigably pursued for more than forty years.

By a great number of special memoirs, published in succession from 1836 to 1858, he applied himself first to making known the plants and insects observed in the three horizons of the Miocene in different localities in Switzerland. Among the local floras and faunas thus established, unquestionably the richest, and also the most instructive, is that of the lignites of Eningen, belonging to the Upper Miocene, and comprising 475 species of plants. Having accomplished this, he compared and coordinated the rich material thus acquired for science in two great works forming a whole :—the *Faune des Insectes tertiaires*, of which I have not to speak here ; and the *Flore tertiaire de la Suisse*, in which are described, figured, and classified 920 species of fossil plants, of which 700 were new.

A little later, completing his investigation of the Miocene plants of Switzerland, extending his researches to the vegetables of all the tertiary formations of Europe, and adding the results obtained respecting the plants of the same age found in the other regions of the globe, he endeavoured to reconstruct the world of which those creatures formed a part, and gave us his grand work entitled *Recherches sur le Climat et la Végétation du Pays tertiaire*, a French

edition of which was published in 1861. Since that period, pursuing without intermission the study of the Tertiary plants discovered in divers countries till then unexplored (for example, the Isle of Wight, Greenland, &c.), he laboured incessantly, adding some general features to the magnificent work to a sketch of which we here limit ourselves.

Let us first place ourselves in Switzerland, where, as we know, the various freshwater formations belong to the three horizons of the Miocene. The Tertiary flora of Switzerland comprised at least 3000 species of plants. It was consequently much richer and more varied than even that of the most favoured countries of the south of Europe; and we must go into tropical regions—to Jamaica, for instance, and to Bahia—to see at the present time such an abundance and diversity of forms. Assembled at that time within the little territory of Switzerland, those plants are now disseminated into all parts of the world; but it is in America, and especially in the southern United States, that most of them are at present found. If, instead of the number of species, we consider the mass of the vegetation, Miocene Switzerland still less resembles the present Europe, and comes nearest to America by its abundance of evergreen oaks, maples, and poplars, by its plane-trees, liquidambers, *Robinia*, *Sequoia*, *Taxodia*, and ternate-leaved pines—to Japan by its *Camphora* and *Glyptostrobus*, and, lastly, to the Atlantic islands by its laurels. It has nevertheless a character peculiar to itself, which is now found nowhere on the surface of the globe—a character expressed at the same time in that singular combination of specific types at present separated by great distances, and by the existence of certain very peculiar species which have become extinct.

From Switzerland let us pass to Europe, in order to compare the characters of the flora in the various successive Tertiary formations. We shall there see striking differences, which impressed a peculiar physiognomy on the vegetation of each period. Thus, in the Eocene flora Indo-Australian types predominate, the American species being but very feebly represented, and the characteristic plants of the temperate zone totally wanting. The Eocene flora was therefore essentially tropical.

The flora of the Lower Miocene has a subtropical character—though tropical forms are still numerous, and the Indo-Australian types still play an important part. The generality of the species, however, belong at the present time to the subtropical and warm zones; and, above all, the forms of temperate climates are seen to make their appearance there. Besides, both the subtropical and the temperate forms correspond with American types, and give to the flora an American tint.

At the period of the Upper Miocene the tropical types have not yet disappeared; but they are reduced to about 7 per cent. of the total vegetation, while those of the temperate zone rise to 18 per cent. The forms of the subtropical and warm regions still predominate. The American character of the vegetation is expressed still more plainly and evidently.

In the Pliocene flora the tropical types have totally disappeared; but a few subtropical species are still found. The plants of the warm region predominate; those of the temperate zone continue to increase in number. The American character remains very marked.

Finally, in the Quaternary flora the subtropical species and those of the warm region have entirely disappeared, even in Italy. Most of the plants are identical with those now living in the same regions. Some exotic and extinct types, however, are found in it, and as it were an echo of America.

We see, then, in short, how the present flora has issued little by little from the tropical Eocene flora—how, by degrees, the forms of warm climates, then those of temperate ones were added to the tropical forms, which retired before them in the same proportion, leaving their rivals to constitute alone the modern vegetation. We see also that in the beginning it is the Indo-Australian types that compose the flora; but as the element which is gradually added has a very decided American complexion, the vegetation assumes, in proportion as it is augmented, a more and more American character, which afterwards wears off, and at the diluvial period again, to a great extent, disappears.

All these researches, both upon the fossil plants and animals, coincide in obliging us to regard the climate of Europe generally as warmer during the Tertiary period than at the present time—as subtropical, similar to that of the southern United States, and specially Louisiana. But in a given locality there were differences according to the periods, and at a given period differences according to the localities. Thus, at the Upper-Eocene period, the mean temperature in any part of Europe was probably 13 or 14 degrees higher than it is now in the same locality; at the period of the Lower Miocene the difference was only 9 degrees, during the Upper Miocene 7; and in the Pliocene period it fell to 3 degrees. And thus, again, at any one period the climate was far from being the same in the different parts of Europe: there was a distribution of the heat according to certain zones; and all the researches, especially the study of the fossil plants of Iceland (which is particularly instructive in this respect), agree in demonstrating that that distribution took place in precisely the same manner as at the present day.—*Annales de Chimie et de Physique*, October 1878, t. xv. pp. 157–161.

ON THE DIFFUSION OF LIQUIDS. BY J. STEFAN.

In this memoir the observations made by E. Voit and Hoppe-Seyler on the diffusion of sugar-solutions, by the saccharimetric method, are first discussed, and compared with the theory of diffusion advanced by Fick. Of these experiments those instituted by Hoppe-Seyler on the diffusion of urine-sugar accord best with the theory; and from them the coefficient of diffusion can also be determined, which is found = 0.42, taking the centimetre as the unit of length, and 24 hours as the unit of time.

Hoppe-Seyler's observations, made with another saccharimeter,

on the diffusion of cane-sugar, as well as the analogous experiments of Voit, show such wide deviations from the theory that the calculation of them according to the formulæ derived from it has no meaning.

Further, the experiments instituted by Johannsjanz, after the prism method described by Kundt, are considered, of which the formal differences from the theory do not appear to be great; yet the coefficient of diffusion of common salt through water, calculated from them, namely 0.45, is too little by more than half. From the determinations made by Fick upon the same salt, the values 0.94 for 15° and 1.13 for 20° temperature are deduced for this coefficient; and these values agree with Graham's experiments, as well as with some to be subsequently published.

The great errors by which the results obtained by optical methods are affected proceed from the fact that the hypothesis on which those methods are based, viz. that a horizontal beam of light which falls upon a vertical plane bounding a diffusing liquid remains horizontal during its passage through the liquid, is incorrect. Such a liquid, bounded by two parallel sides whose density diminishes from below upwards, behaves like a prism whose refracting edge is directed upwards; or, inasmuch as the diminution of density of the liquid is not uniform from below upwards, it exhibits, together with the properties of a prism, also those of a cylindrical lens. Several experiments are described in the memoir by which these peculiarities of solution are proved.

The author, in conclusion, refers to the analogous behaviour of sound when it is propagated in or against the direction of a wind the velocity of which increases upwards, from which behaviour Stokes first explained the fact that in the former case a sound is heard at very great, in the latter at only very short distances.—*Kaiserliche Akademie der Wissenschaften in Wien, mathem.-naturw. Classe*, Dec. 5, 1878.

ON THE SPECIFIC HEATS AND HEAT OF FUSION OF GALLIUM.

BY M. BERTHELOT.

1. M. Lecoq de Boisbaudran having had the kindness to place at my disposal an ingot of gallium weighing 34 grams, I have determined its specific heat in both the liquid and the solid state, and its heat of fusion. I worked according to my usual methods and with the aid of my water calorimeter. It is known that gallium fuses at +30°, but may be kept liquid, in the state of superfusion, down to near zero.

2. Two trials, one made between 119° and 13°, the other between 106° and 12.5°, gave 0.0802 as the value of the specific heat of liquid gallium.

3. The specific heat of solid gallium, between 23° and 12°, was found to be equal to 0.079.

This quantity must not be measured too near the melting-point. Two trials made between 28° and 13°, great care being taken not to heat the metal above 28°, in order not to melt it, gave the abnormal values 0.275 and 0.352; but I perceived, at the same time,

that the fragments of the metal were soldered together at places, under the influence of a partial softening: these numbers therefore include a portion of the heat of fusion.

4. The heat of fusion of gallium can be easily determined. By introducing some crystals into the superfused gallium, the whole of the metal crystallizes rapidly. At 13° I thus found $+19.14$ and $+19.08$, mean $+19.11$, for the unit of weight.

This number remains sensibly the same for every temperature between 30° and zero, on account of the specific heats of the liquid and the solid being nearly identical. Referred to the atomic weight, it becomes 1.33 calory.

5. It will be observed that the two specific heats of gallium, taken near the same temperature, are nearly identical. Mercury presents the same peculiarity, its specific heat being, according to Regnault:—

0.0319 between -40° and -78° ;

0.0333 between zero and 100° .

It is the same with the other metals. Thus the specific heat of melted lead between 350° and 400° , according to Person 0.040 , exceeds by only one fifth that of the same metal, solid, at the ordinary temperature, or 0.032 .

The same with tin (0.056 cold, 0.063 at about 300°).

The same with bismuth (0.031 cold, 0.036 at about 320°):—slight deviations, attributable in great measure to the difference of the temperatures, as the specific heats go on increasing with the temperature. It may be assumed that the solid and liquid specific heats of all these metals, if taken at the same temperature, would have values very near one another.

6. The atomic weight of gallium, recently determined by M. Lecoq de Boisbaudran, being 69.9 , its specific heat in the liquid state is equal to 5.59 , in the solid state to 5.52 .

This product is the same for aluminium, or 5.53 (Kopp), and for glucinium, according to MM. Nilsson and Pettersson's new measurement, or 5.64 .

With these may be compared the analogous metals, such as zinc, 6.08 (Kopp), and magnesium, 5.88 (Kopp).

Manganese (6.69), so similar to the last metals, and crystallized silicium (4.62), of which the oxide and chloride, on the contrary, remind us of aluminium, give products which deviate much, and in opposite directions, although by quantities nearly equal, when compared with the atomic specific heat of aluminium: the total deviation here amounts to nearly 50 per cent.

I shall not here repeat what I have had occasion to say in this respect relative to the limits of theoretic and practical uncertainty of the law of Dulong and Petit in its applications to solid elements (see *Comptes Rendus*, t. lxxxiv. pp. 1261–1276). In reality that law only presents a precise and incontestable meaning for the simple gases, the only bodies for which it is permissible to assume that one and the same rise of temperature answers to one and the same increase of *vis viva* under the same volume.—*Annales de Chimie et de Physique*, October 1878, t. xv. pp. 242–244.

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XI. *Investigations on the Nature of Spectra* (1. *Theory*;
2. *Spectra of Mixed Gases*). By EILHARD WIEDEMANN*.

SINCE the path-opening investigations of Bunsen and Kirchhoff the spectra of incandescent gases have been subjected to a more searching elaboration; and it has been repeatedly endeavoured to ascertain theoretically the reasons for the occurrence of line and band spectra, to discover relations between the individual lines which compose them, as well as to explain the variations which they undergo through pressure and change of temperature.

As, however, the physicists in question mostly enter more closely only into particular points, and but cursorily touch upon the causes for the spectral lines without bringing them into connexion with other data resulting from the mechanical theory of gases, I have tried, partly in continuation of their considerations, to form for myself a theory of spectral phenomena, which should be useful in enabling me to get, first of all, fixed points for a series of experimental investigations, the first part of which I take leave to communicate supplementary to the above theory.

THEORY.

According to the kinetic theory of gases, the individual molecules contained in them move rapidly in all directions. As Stefan and Van der Waals have inferred from the experiments of Joule and Thomson, between these molecules attractive

* Translated from a separate impression, communicated by the Author, from Wiedemann's *Annalen*, vol. v. pp. 500-524.

forces exist to distances which are great in comparison with the dimensions of the molecules themselves. At very minute distances, however, the molecules must repel one another, since otherwise there would be no reason for the recoil of the particles after each collision. These repulsions probably proceed from the envelopes of æther surrounding the molecules of the bodies, and must diminish more rapidly with increase of distance than the attractive forces. The rotation and oscillation of the individual atoms in the molecule about a common centre of gravity, which take place in addition to the translatory motions, lead, at a sufficiently elevated temperature, to the division of the molecules into their atoms. These rotatory and oscillatory motions are periodic, and must also call forth periodic vibrations in the surrounding luminiferous æther.

Line Spectra.—Let a rarefied gas be heated to the highest possible temperature; we can then assume that the individual molecules are decomposed into their atoms. On the collision of these, oscillatory motions only will occur, since, according to the experiments of Kundt and Warburg on the specific heat of mercury vapour, as well as the theoretic reflections of Maxwell, Watson, and Boltzmann, in monatomic molecules the *vis viva* of the motions of rotation is *nil*. The spectra which make their appearance with these elevated temperatures consist of separate bright lines, the cause of which we have accordingly to seek in the oscillatory motion of the atoms, since they also occur with the vapours of mercury and cadmium, which are regarded as monatomic.

Let the gas be so much rarefied that the time which elapses between two collisions is very great in comparison with that during which the particles are within their reciprocal spheres of action; then, with the motions of the individual æther particles, at first with small amplitudes, a limited number of rays will be emitted of a different vibration-period, whose wavelengths will depend on the special arrangement of the æther among the atoms. If the elongations become greater, say by our raising the temperature of the gas, and with it the *vis viva* of each of the colliding atoms, then to these fundamental vibrations certain harmonic ones will be added, the vibration-period of which, again, will depend on the arrangement of the æther and the forces in action between it and the material atoms.

That many of the individual lines in the observed spectra may be regarded as really harmonics of a fundamental vibration, Lecoq de Boisbaudran, Stoney, Soret, and others have proved by careful calculations. Above all must the intensity

of the higher harmonic vibrations, to which the more-refrangible rays correspond, increase with the augmentation of the amplitude; and in fact Lecoq de Boisbaudran finds that, in the nitrogen-spectrum, at a higher temperature the blue lines, corresponding to the double octave of certain vibrations, come out, at a lower the red and yellow, as a fifth of the same vibrations. Similarly, lithium chloride in the flame of the Bunsen burner shows a very bright red and a very faint orange line; before the blowpipe the latter increases in brightness much more than the former, though without becoming equal to it; if, however, the induction-spark strikes upon a solution of lithium chloride, the orange line is much brighter than the red, and the blue lithium-line becomes very bright. Many other instances might be mentioned.

By mere elevation of temperature, also, as F. Lippich and afterwards Pfaundler have argued on theoretical grounds, a widening of the lines may also be produced by the molecules of the gas having each a high velocity, partly directed towards the observer, partly away from him. In far higher measure, however, do such widenings of the lines take place when we increase the pressure of the gas; the individual vibrating æther envelopes of an atom can then only for a brief period carry on their motions undisturbed, since they are mostly within the compass of the sphere of action* of the other atoms. This conclusion is confirmed by the experiments of Wüllner, G. Camician, and others.

Interferences accompanying great Differences of Progress.—The time during which an optic motion of the æther of the individual backward-and-forward-rushing molecules and atoms takes place undisturbed I have endeavoured to ascertain by the following considerations:—

If two rays of light are to interfere, they must start from the same point; and during the time which elapses between the instants when the first and the second ray are emitted no

* The diameter of this action-sphere is the length to which, reckoned from the centre of the atom or molecule, the æther possesses a state differing from that which it possesses in free space; as the temperature rises the length will increase at those molecules which are composed of a plurality of atoms. On the other hand, the so-called molecular diameter denotes, in the kinetic theory of gases, the distance up to which the centres of two molecules can approach each other when they strike one another—a distance which essentially varies with the force of the collision, and hence may diminish with rise of temperature, as the experiments on the friction of gases show. The diameter of the sphere of action will in every case be greater than this molecular diameter, since the particles pushing against one another must first pass through a portion of the action-sphere before their motion reverses its direction.

disturbance of the vibratory motion can occur in the place in question; at other times sudden changes of phase appear, by which the vibrations are altered in a quite indeterminate manner. Hence that difference of phases up to which interferences are still observable gives a measure for the time during which a regular motion takes place at the luminous point. The source of light is formed of the great number of atoms or molecules which lie in the vicinity of that point. Their internal vibratory motion will in every case remain undisturbed only so long as they do not come into one another's sphere of action—that is, during the time, nearly, that elapses between two collisions. But this period, for the different molecules of the same gas, lies between *nil* and infinity. A preponderating number of them will within a certain very short time experience no collisions. Therefore the rays emitted at the beginning and end of this period from all these particles may interfere, and only those issuing from the few others illumine regularly the field of vision in the apparatus used. Hence the interferences are sharp. But the longer the time which elapses between the emission of the two interfering rays, and the higher the interference-bands we observe, the fewer molecules contribute to bring them about, the more molecules illumine the field of vision regularly, and the less sharp do the bands become. Lastly, with phase-differences corresponding to intervals of time greater than that which is necessary for passing through the mean path-length, they will very quickly disappear, since most of the molecules collide within this period and thus undergo irregular changes of phase.

There are measurements by Fizeau and Foucault, and more recently by J. J. Müller, respecting the higher interferences. With sodium-light, interferences corresponding to a difference of path of more than 50,000 wave-lengths were not to be seen; with hydrogen those of 20,000 were distinctly visible, and the highest possible phase-difference was not yet reached. Therefore, with sodium the vibrations must remain regular up to 50,000 double vibrations at the most—that is (since the sodium-line corresponds to about 500 billion vibrations in a second), during the time

$$T = \frac{50000}{500 \cdot 10^{12}} = 10^{-10} \text{ second,}$$

or during a 10000-millionth of a second. We find the time τ between two collisions, if we divide the mean length of path of the gas in question by the mean velocity.

If (since we can only have to do with the order of magni-

tude of the individual numbers) we lay down for a base the mean length of path of the hydrogen molecule at 0° ($L=194 \cdot 10^{-7}$ centim.) and the mean velocity of the same at the same temperature ($c=1698 \cdot 10^2$ centim.), we get

$$\tau = \frac{L}{c} = \frac{194 \cdot 10^{-7}}{1698 \cdot 10^2} = 1 \cdot 14 \times 10^{-10} \text{ second.}$$

The two quantities T and τ agree relatively so well with one another that at all events the assumption is not inadmissible that the æther particles on the sodium atom may accomplish undisturbed, on the average, as many as 50,000 vibrations. This investigation shows, at the same time, that we need not in all cases conceive of the vanishing of interferences as proceeding from a widening of the lines of the spectrum.

The measuring of the high interferences must also furnish us with a means of determining the amplitude of the æther-vibrations, and therewith the density of the æther. If the length of path in which a particle undergoes no disturbance be, say, x millimetres, if the number of the vibrations executed upon this path be m , then the motion is disturbed just at the commencement of the m th vibration; the m vibrations, for which there is no reason that they should all be perpendicular to the progressive motion of the molecules, are distributed equally over x millims.; the mean magnitude of the amplitude in that direction amounts to $\frac{x}{m}$ millim.

The distance upon which the molecules undergo no disturbance it must be possible to determine by heating the gas under different pressures to the same temperature, and determining the number of the interferences. The length of the molecular diameter, S , is independent of the pressure. But if at the density 1 the mean distances of the molecules are A millim., they are $\frac{A}{2}$ millim. at the density $\frac{1}{2}$; the numbers q and q_1 , however, of the resulting undisturbed vibrations are determined by

$$q = \frac{A-S}{VT} \text{ and } q_1 = \frac{\frac{A}{2}-S}{VT},$$

if T denotes the duration of a vibration, and V the velocity of translation of the molecules. Hence, if q and q_1 be known, A and S follow immediately.

The available data are nevertheless not yet sufficient for carrying out these calculations, the principle only of which is

here intimated, though I hope soon to be able to communicate experimental data for the solution of the above-mentioned problem.

If we compare the line spectra of chemically similar elements, such as potassium, sodium, calcium, and rubidium, they show (as was first remarked by Lecoq de Boisbaudran) the same groups of lines, only in different places in the spectrum. Hence they are composed of the same harmonic vibrations of different fundamentals; therefore the configurations of the envelopes enclosing the material atoms, or the forces acting upon these, must be similar. A displacement of the lines corresponding to one another in the spectra in question from the red towards the violet signifies, *ceteris paribus*, an increase of the attractive force of the atoms upon the æther envelopes enclosing them.

Band Spectra.—If we consider gases which are composed not of single atoms but of molecules, we shall then often have to investigate not the spectra of emission, but of absorption, since at the temperatures at which those gases begin to be luminous their molecules are already broken up. Whether the molecules be composed of homogeneous or heterogeneous atoms, the spectra will always be conditioned by the rotatory or oscillatory motions either of the entire molecules or of the atoms in them or the æther envelopes enclosing the latter; hence they will possess a corresponding character. Thus, with simple gases at low temperatures the so-called band spectra appear, quite analogously to which the spectra of combinations are composed. Both consist of broad bands of light, which upon closer examination prove to be formed of bright and dark lines. So it is with nitrogen, and with carbonic acid; just so, as I have repeatedly convinced myself, do the haloid compounds of the metals of the alkaline earths, and similarly also those of mercury (*conf.* the experiments of Mr. Pierce in a memoir that will shortly appear).

We will inquire first whether these spectra are to be attributed to the rotatory motions of the entire molecules*.

The fundamental vibration of the rotatory motion we can calculate approximately by means of the mechanical theory of gases. Let m be the mass of a molecule, u the velocity of

* A similar view was conjecturally expressed by Helmholtz in a paper by J. Moser. Lockyer likewise refers the different spectra of the same body to differently complicated complexes of atoms. The question whether to the same elements only one (a line spectrum) or a plurality of spectra (of lines and bands) can belong, can be decided in favour of the latter view by Wüllner's last experiments, as well as by Lockyer's investigations on the absorption-spectra of differently heated vapours.

translation, v that of rotation, then (neglecting the oscillatory motion)

$$\frac{\frac{1}{2}mv^2}{\frac{1}{2}mu^2} = \alpha.$$

α can be calculated from the ratio of the specific heat at constant pressure and volume, and, for the permanent gases, is 0.66 nearly; so that the velocity of the rotatory motion,

$$v = u\sqrt{0.66} = u \times 0.813.$$

Further, let the radius of the molecule be r , the number of rotations executed in unit time x , while v is the velocity corresponding to the square of the mean velocity of the particles contained in the molecule. If the entire mass of the molecule were situated at the extremity of the molecular diameter $2r$, then would v be the periphery-velocity, and the number of the rotations in unit time

$$x = \frac{v}{2\pi r} = \frac{u \cdot 0.813}{2\pi r}.$$

As, however, this is not the case, but there are also mass-particles nearer to the centre, the real periphery-velocity must be greater than the mean value v (and the number of rotations, x , therefore greater), yet always of the same order as the value which results from the above formula. An exact determination could only be obtained if the shape of the molecules were perfectly known.

For ordinary temperatures, with hydrogen $u = 1698$ metres, with nitrogen 453 metres; r with the same two gases, according to Rühlmann's calculation of Regnault's experiments in the way indicated by Van der Waals, $= 20 \times 10^{-11}$ metre and 17×10^{-11} metre.

For hydrogen we have thus, very nearly,

$$x = 11 \times 10^{11}, \text{ therefore a billion;}$$

for nitrogen, about

$$3.4 \times 10^{11}.$$

To every rotation of the at all events not spherical molecules corresponds a displacement of the æther, or a fundamental vibration. Mostly, however, it does not lie within the region of the visible rays; but probably we can observe certain harmonic vibrations, perhaps those which follow one another with from 500 to 1000 times the velocity. That in fact, on the occurrence of band spectra, harmonic vibrations to over the 700th are visible has been proved by the calculations of Stoney and Reynolds for the spectrum of chromium oxychloride; they have also shown that, with a suitable choice of the constants, rhythmic arrangements in the degrees of brightness of the

different lines can be recognized as characterizing the band spectra.

The assumption, however, that the band spectra are generated by rotatory motions*, is opposed by the consideration that, if when the temperature is raised the light emitted by gases always consists of rays of the same wave-length, the molecular diameter must increase proportionally to the absolute velocity, or to the root of the absolute temperature, since only then could the number of rotations remain invariable. In all other cases the lines must be displaced towards the red or the violet, according as the molecular diameter increases with rising temperature more or less quickly than the velocity of rotation. That such displacements do not occur we learn from all the spectrum-experiments hitherto made.

Experiments, subsequently to be communicated more fully, have shown me how wide are the limits within which the bands retain their situation. In those experiments nitrogen was heated by the induction-spark to 4000° and 20000° respectively, and yet there were no alterations of position in the band spectrum. The temperatures were determined calorimetrically. It is therefore in the highest degree improbable that the above spectra ought to be ascribed to the rotations of the molecules. Perhaps the frequently occurring augmentation of brightness of the background from which the spectral lines stand out may be referred to them.

Spectra of Compounds.—In order, therefore, to explain the band spectra of the elements, and the spectra (composed quite analogously to them) of chemical compounds, we assume that they are produced by the vibrations of the atoms in the molecule or of their æther envelopes. That, at the same time, the spectra of the compounds are so much more complicated than the (line) spectra of the free atoms can cause us no surprise.

If we further compare the spectra of the undecomposed compounds with the *band spectra* possessed by the elements which compose them, we find that the position and grouping of the lines, as was to be expected, are essentially different in the former from what they are in the latter; for indeed the

* Lecoq, indeed, is inclined to account for both band and line spectra by rotations of the molecules about centres not precisely determined, the motion following elliptical paths, these centres again rotating about other centres, and so forth. By admitting several such centres, and by a more precise determination whether the motion in the various ellipses takes place in the same or in opposite directions, he succeeds in explaining the various observed phenomena; but he gives no physical reason, deduced from other well-known phenomena, for the assumption of that sort of rotatory motions.

forces conditioning in these the vibrations of the æther are of an intrinsically different nature from those in the molecules built up out of the atoms of the same elements; but when the grouping of the atoms in the compounds and in the molecules of the elements composing them are similar, the general appearance of the spectra given by both will also be the same, even if the individual lines in them do not coincide. Thus it is with the absorption-spectra of iodine vapour, vapour of bromine, and the vapour of monochloride of iodine closely investigated by Roscoe and Thorpe. In like manner must compounds of analogous composition also furnish similar spectra—as, for example, the haloid compounds of the earth-metals, mercury, &c.

If we can succeed in determining for the band spectra of the elements or compounds the fundamental vibrations, and if the motions are conditioned by the forces in action between the atoms, we must be able to determine these in absolute and relative measure from the absolute weight and mutual distances of the atoms, which are at any rate determinable by the newer theory of gases. Intimations for such investigations we find in the displacements which, according to Mitscherlich's, Lecoq de Boisbaudran's, and others' observations, certain groups of lines undergo on the transition from a chlorine to a bromine or an iodine compound. From the thermal processes alone which accompany the formation and decomposition of the compounds this is indeed not possible, because, even when we let the elements act upon one another in the gaseous state, the quantities of heat that appear are still conditioned by the indeterminate separation of the atoms which form the molecules of the elements.

As we ascribe the band spectra to combined, but the line spectra to the atoms isolated at higher temperatures, it can be alleged that *cæteris paribus* the former more readily occur with bodies which are with more difficulty subject to chemical actions, and therefore to decompositions, than with less-stable bodies. G. Wiedemann has pointed out that by an electric discharge which corresponds to the compensation of equal differences of potential, equal amounts of heat are generated when it passes through different gases which are present in the same capillary tube and under equal pressure. If these gases (for example, hydrogen and nitrogen) possess equal specific heat even at the temperatures to which they are heated by the discharges, and disintegration does not occur, the temperatures attained must also be equal. Nevertheless nitrogen, which is chemically more stable, shows the band spectrum, hydrogen the line spectrum. Chlorine, bromine, and iodine,

whose great capacity of reaction indicates that their molecules are easily broken up, almost constantly show line spectra. That different substances at the same temperature possess different kinds of spectra we might also be inclined to infer from the fact that in the same Geissler tube, which contains at the same time hydrogen and nitrogen, the nitrogen bands appear together with the hydrogen-lines. This inference, however, is not absolutely correct, since, as we shall subsequently see, in a mixture of gases the passage of electricity is brought about with different degrees of facility by the molecules of different substances.

In complete accordance with this are the results obtained by M. Wüllner, who found, when he caused discharges to pass through gases at a determined pressure, that now line and now band spectra appeared, according as the equalization of the electricities ensued as a tuft or a spark. The latter extended to only a few, the former to great number of particles. As in general at each discharge of a definite collecting-apparatus with an equal charge an equal quantity of electricity passes, the temperature in the spark must be much higher than in the tuft; hence in the spark discharge a breaking-up of the molecules into their parts is much more probable than in the tuft discharge*. Moreover the behaviour of mercury vapour proves that a band spectrum is not constantly combined with the tuft discharge, nor a line spectrum with the spark discharge. If mercury is introduced into wide tubes, and heated, and the discharge of the induction-coil passed through, the luminous particles of mercury fill the entire tube; and yet it shows only the line spectrum. Sodium behaves in just the same manner.

Disagreeing with the views we have now unfolded, some have supposed that the differences between band and line spectra could be explained by mere alterations of pressure or equivalent alterations in the thickness of the luminous layer of gas. This idea, however, is hardly tenable in the face of Lockyer's experiments, who introduced into a glass tube, 5 feet in length, through which a slow current of hydrogen was passing, a small piece of sodium, heated the entire tube to redness, and let the light of an electric lamp pass through its length, which he then examined with a spectroscope. The

* To decide the question why in certain gases which principally give line spectra (as chlorine, bromine, iodine) spark discharges are much more readily formed than tuft discharges, further experiments are still required. Probably a much higher potential is necessary for the discharge in them than in the other gases, to which, perhaps, the shortness of the path-length of chlorine points.

double line of sodium appeared dark, but not thicker than in a short tube under the same conditions, and much thinner than when the density of the sodium was only a little increased. What was here proved for sodium vapour may in an analogous manner hold good for the other gases, such as nitrogen &c. At all events the above experiment shows that changes of thickness must not be supposed to run parallel with changes of density. In order that this should be the case, the curve which represents the dependence of the coefficient of absorption would have to be perfectly identical with that which renders the disturbance (conditioned by the greater number of the collisions in consequence of the greater density) of the vibrations of the æther envelopes as a function of the number of the collisions (which is extremely improbable), while the widening of the lines would be fixed by the former curve at increasing thickness, by the latter at increasing density of the absorbing layer.

Further experiments, by Lockyer, Schuster, and others, teach besides that sodium, as well as the vapours of other metals, possess different absorption-spectra at different temperatures, yet without the limits within which the thickness of the absorbing stratum varies being very wide. From this may also be explained the recent experiments of Liveing and Dewar, who vaporized sodium in a vertical tube heated below to a white heat, the bottom of which served at the same time as the source of light; they then introduced from above to various depths a tube containing hydrogen, and observed the changes of the spectrum. Thereby, on the one hand, the thickness of the layer through which the rays passed was varied, but, on the other, when the tube was sunk deeper the most extremely absorbent layers had certainly a much higher temperature than when it was lowered to a less depth*.

Spectra of Solids and Liquids.—If we condense gases into the solid or liquid state, the vibrations in the individual mole-

* It might at first appear as if the action of the molecules in a body upon a luminous ray which passes through it (as exemplified in dispersion, absorption, the rotation of the plane of polarization) were not proportional to the number of the acting molecules in the unit of volume (as, we learn from experiment, it is), but to the number in the unit of length, and therefore to the third root of the former. But if we employ a method analogous to that which serves for ascertaining the number of the collisions of a molecule in a space filled with other molecules of gas, we see that the vibrations of the luminiferous æther are affected by all the molecules which are present in a cylindrical space whose radius is equal to the radius of action of a molecule upon an æther particle. But the number of molecules in such a space increases proportionally to the total number of the molecules.

cules almost never continue undisturbed; in the absorption-spectra (to the examination of which we are here almost exclusively directed), sharp absorption-streaks appear only in isolated cases—with the salts of uranium, didymium, and substances of analogous composition to potassio-chromic oxalate. The last-mentioned shows in its absorption-spectrum, beside a broad band in the orange and yellow, a sharp black line in the red. The absorption of single rays or groups of rays corresponds to the vibrations of the atoms composing the molecules or their proximate constituents. Hence, if a group of atoms occurs in different combinations, it calls forth in general the absorption-spectrum corresponding to it (nitrocompounds, chromates), which, however, may be more or less modified by the presence of other atoms with those occurring in the molecules. Thus all the salts of didymium show on the whole the same absorption-spectra; but the individual lines are somewhat displaced, sometimes towards the violet, sometimes towards the red, according to the nature of the acid. Upon the atoms of the absorbent part of the didymium group the acid evidently exerts an attraction which does not extend equally to all of them, and therefore does not produce a mere shifting of the centre of gravity of the system. Then, according to the position of the acid and the strength of its attraction, the vibration-period may be increased or lessened.

Nevertheless this is not always the case. Thus experiments made at my suggestion by M. Pierce, with the chromic oxalates of potassium, sodium, lithium, and silver, showed that the sharp streak in the red, quite independently of the nature of the shifting constituent, the metal, always retains the same position, even when the solution is heated. We must therefore admit that the action of these metals upon the absorbing group of atoms is unperceivable.

Pleochroism.—In a similar way may pleochroism be explained. The vibrations which take place in the molecules that build themselves together in a definite manner to form crystals are affected very differently by the neighbouring molecules, according as they take place in one or the other direction; or else, in molecules thus arranged, the motions in different directions have from the beginning different fundamental vibrations. To derive pleochroism at once from differences in the dispersion of the different rays is surely not admissible, since the different dispersion is, according to the newer theories, just a consequence of difference in the strength of the absorption; on the contrary, a connexion between the two quantities is certainly to be expected.

Displacement of the Spectra in Solutions.—I am inclined to attribute, at least in part, to chemical influences those displacements which the absorption-streaks of various materials undergo according to the substance in which they are dissolved—especially in the case of organic colouring matters, the quickly commencing decomposition of which in solutions indicates the chemical influence of the solvents. The influence of the dispersion of the solvent may be checked, since solutions, for instance, of potassio-chromic oxalate in water and in glycerine show the absorption-streak in exactly the same place in the spectrum, although the dispersions of these two substances are sensibly different.

The question, finally, which has lately been repeatedly discussed, of the difference of the spectra, both of emission and absorption, of one and the same substance under different conditions (H. W. Vogel and J. Moser), it appears to me may be solved thus (provided the above considerations are true):—Within the molecules of any chemical compound, which are withdrawn from the influence of the molecules in their vicinity, perfectly definite vibrations take place, conditioning completely determined spectra; but as soon as these molecules come nearer to one another, or, in liquids or solids, form more complex (multiple) molecules, or, finally, in solutions are influenced also by the action of another substance, the spectra may change their appearance. That two different substances give perfectly identical spectra is thinkable only when the forces acting between the atoms in their molecules are identical.

It is my intention to test by experiments, and pursue further, the views which have now been unfolded, and which very well explain a great number of the facts that have hitherto been observed. But for this, it is before all things necessary to examine the spectra under completely determined conditions of temperature and pressure.

The most convenient means for the production of elevated temperatures, such as are necessary to call forth gas-spectra, is, decidedly, the electric spark. Hence the temperatures generated by it must be exactly ascertained; and, above all, we must accurately settle whether it calls forth the luminous phenomena by elevation of temperature alone.

In order first to decide the last point, I tried whether, in mixtures of two gases through which a discharge passes, the spectra of both substances constantly make their appearance, or only one of them.

SPECTRA OF MIXED GASES.

Discharges in Mixtures of the Vapours of Mercury and Sodium with other Gases.

In a Geissler tube filled with hydrogen a little mercury was hermetically enclosed and the tube heated in an air-bath, while at the same time the current of an induction-apparatus passed through it. Whilst at the ordinary temperature the hydrogen-spectrum was obtained, with the heating the lines of mercury were added; these became brighter and brighter as the temperature rose; and at the same time the hydrogen-lines disappeared both in the wider portions of the tube and at the electrodes*.

In order to accurately investigate and with certainty establish this phenomenon multifarious experiments were made. The tubes employed were either of the usual form of a Geissler, consisting of a central capillary part and two wider parts at the extremities, or they were prepared from a wide tube by drawing out and thus narrowing the middle portion before the blowpipe-flame, or else they consisted of a tube of uniform width (about 10 millims. diameter) and 50 millims. length. Finally, the shape of the electrodes (constantly of platinum) was varied: either they were both spherical (diameter 3 millims.), or one spherical and the other pointed, or both pointed.

For the filling and exhaustion of the tubes, two side pieces were joined on by fusing, of which the one was hermetically closed, at about 1 centim. from the wall of the tube, immediately after drying, while the other was connected with the air-pump. This also, as soon as the intended pressure was generated in the tube, was melted off, leaving a length of from 2 to 3 centims. If further measurements were to be instituted at a different pressure from that at first employed, the point was broken off, the tube exhausted, and again hermetically sealed, and so on. With a little care the tube could thus be used successively three or four times.

* This method of producing the spectrum of mercury may be of value in optical investigations in which it is necessary to have homogeneous light of a determined wave-length. While in the red the lines of lithium and hydrogen (of which the blue-green and violet lines can be easily intercepted), in the yellow the sodium-line, in the blue-green and violet likewise hydrogen-lines are at disposal, homogeneous green light is only to be obtained with difficulty, on account of the great volatility of thallium. The mercury-spectrum is distinguished by a very beautiful green line, from which the other, brighter lines (a double yellow and a violet) are sufficiently distant to be covered by a diaphragm or eliminated by suitably coloured glasses.

The tubes were then fixed insulated and horizontal in an air-bath of iron, 16 centims. deep, 11 centims. wide, and 14 centims. long, by means of glass tubes which passed through the two side walls of the bath. The front and back walls of the bath were formed of plates of mica; and another mica plate served as a cover. The heating was effected by a gas-burner placed beneath. The progress of the phenomena was examined both during the heating and especially during the cooling. The temperatures were measured by an ordinary thermometer placed immediately on the Geissler tube.

For the examination of the spectra either a Bunsen spectrum-apparatus or a Browning direct-vision spectroscope was used. To the former, in the place of the hair cross in the observing-telescope, a slit-diaphragm was applied, in order to blind off certain portions of the spectrum.

A middle-sized Ruhmkorff induction-apparatus served as the source of electricity, its primary current being furnished by 3 or 4 Bunsen elements; a small induction-apparatus, however, or a Holtz electrical machine was also employed. In all cases the same phenomenon was shown. At a not too high temperature (between 100° and 200°) the lines corresponding to the gas contained besides the mercury in the Geissler tube disappeared. At the same time a clear difference between the two electrodes was observable. When, on slowly heating, at the positive electrode and in the whole of the luminous tuft issuing from it mercury-lines alone were already to be seen, at the negative hydrogen- and nitrogen-lines were still distinctly shown, which at higher temperatures (at which larger quantities of mercury were present in the gaseous space) likewise vanished.

The best idea of the progress of the phenomenon will be given by the following series of experiments. The tube was of uniform width, the electrodes spherical; the large induction-apparatus served as the source of electricity. The pressure of the enclosed air amounted to about 10 millims. The tube was first heated to 240° , and then permitted to cool slowly.

Above 240° , everywhere only the mercury-lines are to be seen, both in the middle of the tube and at the electrodes. The discharge issues as a zigzag line of light from the fore end of the positive knob, and plays round the negative, without any perceptible dark space between. At the wire to which the negative knob was fastened, no light is to be seen.

Between 230° and 210° the nitrogen-lines, together with those of mercury, begin to appear at the negative electrode; and at the same time the dark space unfolds itself, the wire of

the negative electrode becomes bright, and the positive discharge spreads out into a luminous tuft. At 160° the green nitrogen-lines are distinctly seen, and the lines of mercury become more feebly luminous. At 130° the mercury-lines at the negative electrode are less bright than in the positive luminous sphere. First at 100° do traces of the nitrogen-lines appear in the luminous tuft and at the positive pole, and come out more and more with a further lowering of the temperature.

The same phenomena were displayed at pressures of 30, 60, and 100 millims.; the nitrogen-lines then made their appearance in the positive tuft at temperatures between 100° and 140° . Above 230° , everywhere only mercury-lines could be seen.

In another experiment some mercury was introduced into a Geissler tube containing hydrogen. Before the heating, in the capillary part the hydrogen-lines showed distinctly. On heating, they disappeared, and did not reappear with the cooling. Even when the current was interrupted for some time, they showed themselves only for a moment after its closing. It is probable that, at the first heating and cooling, some traces of mercury were precipitated in the capillary tube. The glass was then heated so strongly by the passage of the electric spark that they were vaporized. Not till the tube was wholly immersed in a mixture of ice and salt, and thereby the glass made very cold, so that the layer of yellowish mercury (oxide?) no longer furnished enough vapour, did the hydrogen-lines reappear.

In order to discover whether other metallic vapours have the same properties as are exhibited by mercury vapour, some sodium was enclosed with hydrogen or nitrogen. Here also, at the heating, the nitrogen- and hydrogen-lines vanish; and this commenced earlier at the positive than at the negative pole. At temperatures at which the glass began to soften, there remained only the yellow, green, and blue double lines of sodium visible. That in these experiments the fused sodium did not chemically combine with the nitrogen present, and that the disappearance of the nitrogen-lines was not conditioned thereby, was made known by the fact that, differing from Salet's experiments, the hydrogen- and nitrogen-lines again made their appearance after the cooling.

If a spectrum-tube filled with nitrogen and hydrogen be suitably heated at one part far below redness, very slight traces of sodium and other metals from the glass are vaporized there; and there also the hydrogen- and nitrogen-lines almost completely disappear, together with the metallic lines that occur.

Very hot spark discharges even might exert similar actions. But we cannot hence infer that the hydrogen itself vanishes or is transformed into a material of another nature.

The above-described phenomena might result from several causes. (1) The vaporized mercury may crowd the other gases out of the hotter portions of the tube into the colder, so that its own vapour alone is found in the path of the spark. (2) The phenomena depend on differences of temperature occurring in the spark itself when the discharge passes through gases mixed with mercury and also through pure gases. (3) They are conditioned by this—that whenever any two gases are mixed, only one of them becomes luminous. (4) Mercury particles are torn from the electrodes, and carry over the electricity in disruptive discharges. (5) Mercury behaves towards electricity essentially otherwise than hydrogen and nitrogen.

To test the first assumption the Geissler tubes were placed vertically instead of horizontally. If this assumption had been correct, the behaviour of the upper must then have been essentially different from that of the under parts—which, however, was not the case. A further argument against this assumption is, that in tubes of equal width throughout the phenomena were as well exhibited as in tubes with a capillary middle piece.

Secondly, that the disappearance of the lines is not conditioned by differences of temperature in the spark itself, follows from the fact that when the most different sources of electricity are employed (which produce very differently heated discharges) it equally occurs, that it is also independent of the nature of the electrodes, and that it is exhibited in the wide as well as in the narrow portions of the Geissler tubes (in which the temperatures differ considerably). Further, if we view in the rotating mirror the discharges of a Holtz machine that take place in a Geissler tube, the intervals between them after the heating, when the mercury-lines appear, are not essentially other than at the ordinary temperature, provided only that the pressure in the Geissler tube has not varied during the experiment—which it was easy to secure by keeping it constantly connected with the air-pump. Since the intervals do not vary, equal amounts of electricity pass in equal times, and the temperatures of the incandescent particles of gas will not be very different.

Thirdly, in order to establish that the appearance of the spectrum proper to nitrogen was not prevented by the addition to it of any considerable quantity of any gas or vapour whatever on the passing of the electric spark, some iodine was

enclosed in one of the tubes of average width, with points as electrodes; the pressure of the nitrogen amounted to about 10 millims. Even when it was heated to 220° , the nitrogen-spectrum was still distinctly visible together with that of iodine—though, it is true, a series of dark absorption-lines proceeding from iodine vapour were drawn through it. As the iodine vapour filled the whole of the tube, the light (which was emitted from the incandescent gas occupying only the middle of the tube) must have passed through a considerable thickness of it. At all events the tension of iodine at 220° is much greater than that of mercury at the same temperature, since the former boils at 180° , but the tensions of the latter are uncommonly small (at 100° , according to Regnault, 0.146 millim., at 140° 3.059, and at 200° 19.19). Nevertheless in the mercury-tubes the nitrogen-lines disappeared, even with 100 millims. pressure of nitrogen, at 140° at the positive electrode*.

Fourthly, that the phenomenon cannot be conditioned by mercury precipitated on the electrodes being disruptively carried away we learn, first, from the experiment described on p. 92; and then, again, it was indicated by observations which were made with glass tubes without internal electrodes, in which the ends were only coated with tinfoil, and yet, when the coverings were connected with the sources of electricity, the nitrogen-lines vanished nevertheless.

To mercury, therefore, must be attributed a behaviour towards electricity which differs from that of hydrogen and nitrogen. We might assume that the mercury molecules alone take part in the discharges, or that they take a preponderating measure of the electrical charge.

In the mercury vapour mixed with hydrogen the discharge perhaps takes place in this way:—There is an accumulation of electricity at the electrodes; and this distributes itself to the molecules of the surrounding gas. But the mercury molecules will be more charged than those of hydrogen; and by them will the discharge be chiefly brought about, because on their encountering other mercury and nitrogen molecules they will in preference give up their electricity to the former. But the passage of the electricity conditions the luminous phenomena; hence only mercury-lines appear.

Since, further, the electrodes do not behave alike, but the nitrogen-lines are much longer visible at the negative than at the positive electrode, we may perhaps assume that mercury charges itself more readily with positive than with negative

* Even mixtures of hydrogen and nitrogen constantly show the spectra of both gases.

electricity, and therefore exerts a more powerful attraction upon the positive than the negative. (On the different behaviours of positive and negative electricities, compare also G. Wiedemann and R. Rühlmann.)

If these experiments show that the passage of electricity from one particle to another takes place in various ways in different substances, and that, independently of the total temperature of the mixture, only certain particles are rendered luminous by the electric spark, we may further have to take into consideration that the passage of electricity from atom to atom is capable of producing oscillatory motions of their æther envelopes, yet without augmenting in corresponding measure the *vis viva* of the progressive motion of all the particles, as would be necessary according to the theory of gases. We find an analogy to this in the augmentation of the interior motion in a molecule without a corresponding increase of the oscillatory motion of the whole when non-luminous discharges pass through different gases: the latter undergo decompositions which otherwise would only be produced by considerable elevations of temperature. Similarly, in the phenomena of fluorescence, by the incident æther-vibrations the *vis viva* of the translatory motions is, for certain vibrations, increased in a way that corresponds to a direct augmentation of the progressive motion by heating to 500° and more.

In order to put these assumptions to the proof, I have already commenced a series of experiments, in which the temperatures of the discharges in Geissler tubes under various conditions are determined.

Leipzig, August 1878.

XII. *An easy Method for Adjusting the Collimator of a Spectroscope.* By ARTHUR SCHUSTER, *Ph.D., F.R.A.S.**

THE ordinary method for adjusting the collimator of a spectroscope for parallel rays is only applicable to the mean rays of an achromatic combination. At the extreme ends of the spectrum a readjustment has to be made. If the ultra-violet rays are observed, and if the lenses are of quartz, the ordinary method cannot be used. The following method is so simple that I cannot help thinking it has often been in use; yet I have nowhere seen it described, and I know that others, like myself, have often found a difficulty in making

* Communicated by the Physical Society.

the adjustment without much loss of time and with simple apparatus.

The adjustment, as the following consideration will show, can be made on each line of the spectrum without any apparatus whatever. The only requirement is that the prism should be movable.

Suppose the rays which fall on the prism to be either convergent or divergent; then, after their passage through the prism they will seem either to converge to or diverge from a point, which is the secondary focus: as the prism is turned, so as to change the first angle of incidence, the secondary focus will change. If the rays are strictly parallel, then, whatever be the position of the prism, the focus will not be altered. This, then, is a delicate test for ascertaining whether rays proceeding from the collimator are parallel or not. It remains to be shown how it can be converted into a rapid method to put the collimator into the right adjustment.

The three fundamental equations for the passage of a ray of light through a prism,

$$\sin i = n \sin r, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\sin i' = n \sin r', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$r + r' = \alpha, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

give

$$\frac{di'}{di} = - \frac{\cos i \cos r'}{\cos i' \cos r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

In these equations i and i' are the angles which the ray makes with the first and second surfaces respectively on entering and leaving the prism; r and r' the two corresponding angles of refraction, and α the angle of the prism. The right-hand side of equation (4) will, as a little reflection will show, steadily decrease when i is increased from 0 to $\frac{\pi}{2}$. This shows

that the greater the first angle of incidence the more nearly parallel are the rays. The following system of consecutive approximation will therefore give the desired result.

Suppose the collimator is out of adjustment: move the telescope slightly out of position of minimum deviation; then two positions of the prism exist which will bring the desired ray into the middle of the field. Call the position in which the first angle of incidence is greatest A, the other B.

1. Put the prism into the position A, and focus the *telescope*

until the line in question, either dark or bright, is distinctly seen.

2. Move the prism into position B, and focus the *collimator* until the same line is distinctly seen.

3. Repeat the operation, always focusing the telescope when the prism is in position A, and the collimator when the prism is in position B. After three or four trials no change of focus is required; both collimator and telescope will then be adjusted for parallel rays. I find that it is by no means necessary to work much out of the position of minimum deviation in order to gain a delicate adjustment. If the adjustment is made in the centre of the field, then I usually put the telescope into such a position that the line, when the prism is placed at maximum deviation, should just be out of the field of view; this gives quite a sufficient change of focus if the rays are not parallel on entering the prism.

The following measurements, which were purposely made without special care, will show the accuracy of which the method is capable. The sliding tube of the collimator was divided into millimetres. Two different adjustments for the sodium-line, made in the way described above, gave the readings 5.0 and 4.0. The prism was now turned round so as to deflect the ray to the other side. Two adjustments now gave 4.1 and 5.0. The mean of the four readings is 4.5. The adjustment was then made according to the well-known method of first focusing the telescope on a distant object and focusing the collimator to the telescope afterwards: the reading was 4.2. As the focal length of the collimator was 300 millimetres, the two results differ only by a thousandth part of the focal length. Whether this difference is due to errors of observation, or whether it is produced by a difference in the focus of the yellow rays and the mean visible rays, I cannot say; but I believe, with a little precaution, the method can be adapted to the study of the achromatism of a lens.

I have assumed that the faces of the prism are perfectly plane. Practically it is difficult to get a prism in which this condition is accurately fulfilled; and it may be questioned whether the curvature of the prism may not seriously interfere with the accuracy of the method. To this I reply:—

1. That a prism which is known to be good may always be set aside to do this work.

2. That the reason of having the rays strictly parallel on entering the prism is based on the supposition that the faces of the prism are plane. It is by no means evident that parallel rays will give the best definition when the faces of the prism are curved.

3. That the change in the adjustment of the collimator introduced by the curvature of the prism is very small. One prism, which I know to be exceptionally bad, gave a difference of a half per cent. in the focal length of the collimator. It is not the change of focus introduced by the curvature of the prism which makes the method inaccurate when the prism is bad, but the difference in the change of focus in the two positions of the prism. This is one of the reasons why it is better to take the two positions of the prism not too far away from minimum deviation. The small displacement of the prism will only introduce a small variation in the focal length due to the curvature of the faces.

XIII. *On the Morphological Laws of the Configurations formed by Magnets floating vertically and subjected to the Attraction of a superposed Magnet; with Notes on some of the Phenomena in Molecular Structure which these experiments may serve to explain and illustrate.* By ALFRED M. MAYER*.

IN the May Number (1878) of this Journal (page 397 of vol. v.), a short note was published on my experiments with magnets floating vertically and subjected to the attraction of a superposed magnet. The object of this paper is to present accurate diagrams of the configurations formed by the floating magnets, and to give the laws ruling these configurations, with some notices of the peculiarities of these forms. At the same time I will show how neatly these experiments illustrate several phenomena in the molecular structure of matter.

The Diagrams.—These diagrams show the configurations formed by numbers of magnets extending from two to twenty. They were obtained as follows:—The number of needles forming a configuration were floated in a bowl filled to its brim with water. The eye-ends of the needles, which protruded a short distance beyond the tops of the corks, were of S. polarity. A cylindrical magnet, 38 centims. long and 15 millims. in diameter, was clamped in a vertical position, with its N. end at the constant distance of 60 millims. above the tip of the needle, which floated in the line of the axis of the magnet. I tipped the ends of the needles with printer's ink; and when the configuration had formed and was stationary, I brought down upon the needles a piece of flat cardboard, and thus obtained prints from nature. Around each of the dots on the card-

* Communicated by the Author.

board I drew a black disk. The centres of these disks I joined by lines, in order to bring before the eye the contours of the configurations. After the diagrams of the configurations had been obtained in this way, they were placed at a fixed distance from the camera; and photo-engravings were thus made, of about one half the sizes of the original prints.

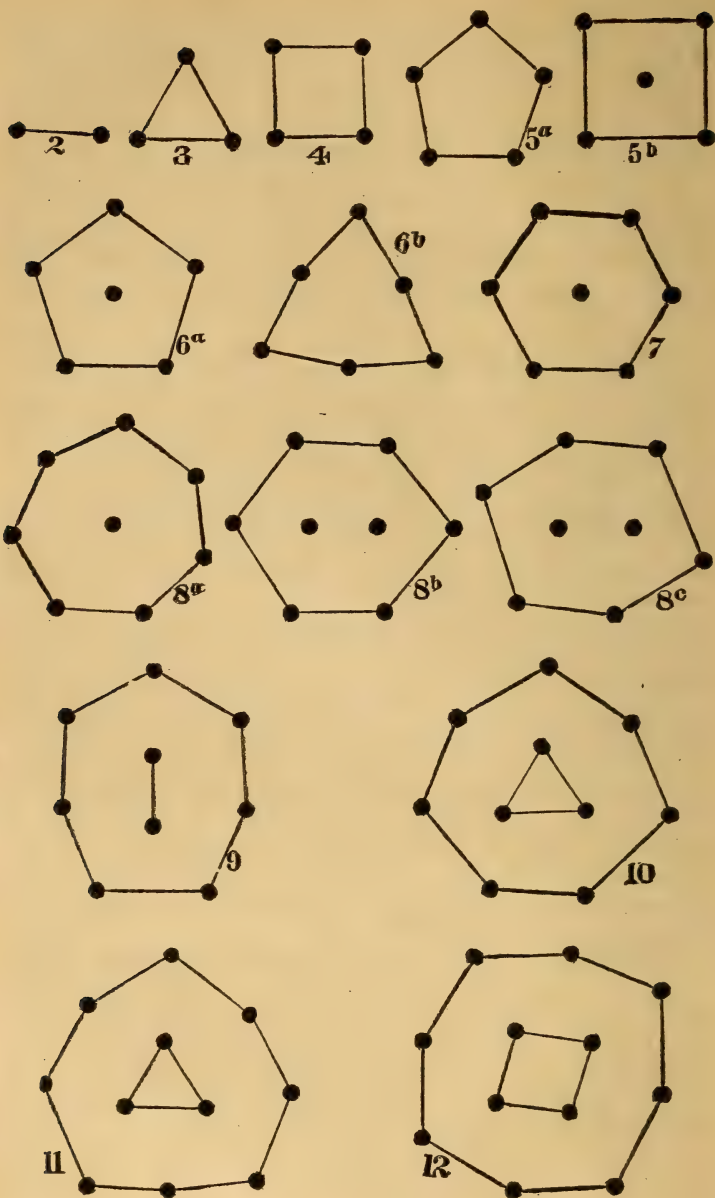
The Morphological Laws of the Configurations.—The configurations made by the floating magnets form well-marked groups or classes, which may be designated in order as primary, secondary, tertiary, quaternary, &c. The stable configurations of one class form the nuclei to the succeeding ones.

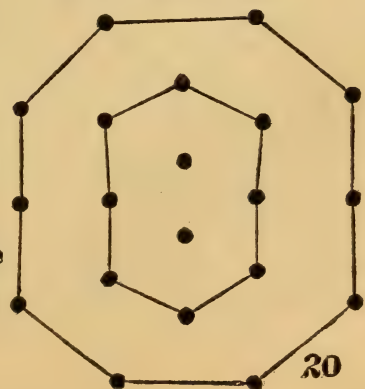
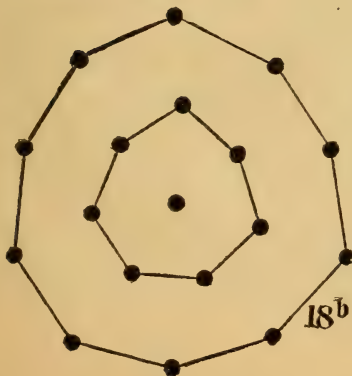
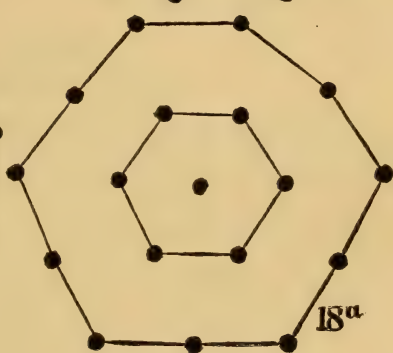
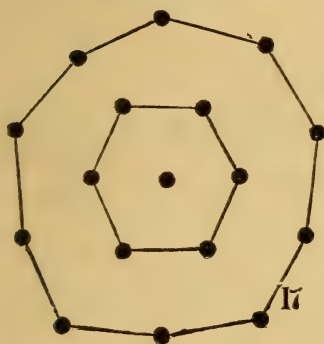
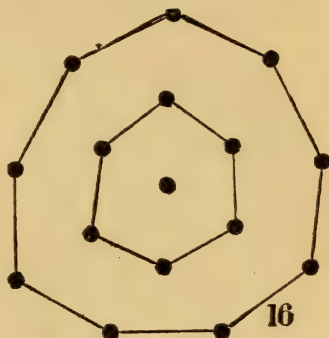
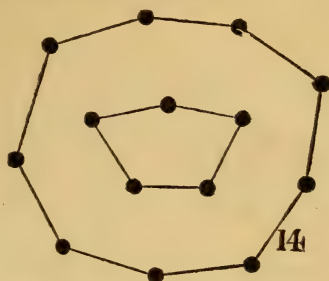
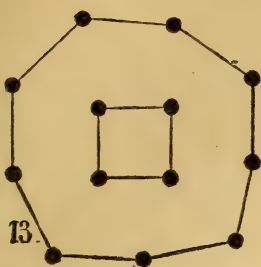
Looking at the diagrams, the reader will see that figures 2 to 8*a* inclusive form the *primaries*. Figure 8*b* begins the secondaries, for it is the hexagon with 2 for nucleus. Configurations 8*b*, 8*c*, 9, 10*b*, 10*a*, 11, 12, 13, 14, 15, 16, 17, 18*a*, 18*b*, and 19*a* are *secondaries*, having respectively for nuclei the primaries 2, 2, 2, 2, 3, 3, 4, 4, 5*a*, 6*a*, 7, 7, 7, 8*a*, 8*a*. The configuration 10*b* is not found in the diagrams; it is the same as the 10 which forms the nucleus of 20, only the two central lateral needles are further removed outward from the vertical axis of the figure 10.

The group of 19*b* begins the *tertiaries*. Of these I do not give diagrams, but indicate their structure by giving the numbers of the secondaries forming their nuclei, and then give the numbers of needles grouped around these nuclei. Thus, the structure of the configuration formed of 47 needles is indicated by $47 = (18 + 14) + 15$; which means that 47 needles form a configuration which has 18 for its inner nucleus, surrounded by 14 needles, and these in turn surrounded by 15; and as $18 + 14$ forms the tertiary which is the nucleus to this quaternary 47, we enclose 18 and 14 in parenthesis.

I here give the configurations to 51 inclusive, which form begins the *quinaries*.

The configurations of the same number of magnets are lettered *a*, *b*, *c*, to indicate their degrees of stability, *a* being always the most stable form. I have, however, lettered the configuration of 8 magnets in the order of their increasing areas, in order to make them serve better for the purpose of illustrating the phenomena of isomerism. Really 8*c* is more stable than 8*b*.





Tertiaries.

$19b = 9 + 10$	$25a = 12 + 13$	$29a = 16 + 13$
$20a = 9 + 11$	$25b = 13 + 12$	$29b = 17 + 12$
$20b = 10 + 10$	$26a = 13 + 13$	$30a = 17 + 13$
$21a = 10 + 11$	$26b = 14 + 12$	$30b = 18 + 12$
$21b = 11 + 10$	$27a = 14 + 13$	$31 = 18 + 13$
$22 = 11 + 11$	$27b = 15 + 12$	$32 = 18 + 14$
$23 = 11 + 12$	$28a = 15 + 13$	$33 = 18 + 15$
$24a = 12 + 12$	$28b = 16 + 12$	$34a = (8a + 12) + 14$
$24b = 11 + 13$		

Quaternaries.

$34b = (9 + 10) + 15$	$43 = (15 + 14) + 14$
$35a = (9 + 12) + 14$	$44 = (15 + 14) + 15$
$35b = (10 + 12) + 13$	$45 = (16 + 14) + 15$
$36 = (10 + 12) + 14$	$46 = (18 + 14) + 14$
$37 = (10 + 13) + 14$	$47 = (18 + 14) + 15$
$38 = (11 + 13) + 14$	$48 = (18 + 15) + 15$
$39 = (11 + 13) + 15$	$49 = (18 + 15) + 16$
$40 = (13 + 13) + 14$	$50 = (8 + 11 + 15) + 16$
$41 = (13 + 13) + 15$	$51a = (8 + 12 + 15) + 16$
$42 = (13 + 14) + 15$	

Quinaries.

$$51b = (9 + 12 + 14) + 16.$$

I do not say that the above list contains all the possible combinations. The list is more for the purpose of establishing the laws which I have already formulated.

In my first publication I gave two configurations for four needles:—one having the needles at the corners of a square, and a stable form; the other unstable, and formed of a triangle containing a central needle. I have concluded that this form does not exist; at least its existence is so transient that it has never remained long enough for me to take a print of it.

I have stated that $19b$ begins the tertiaries. This is an unstable configuration, and is formed of 9 surrounded by 10 magnets. The other 19 , $19a$, is stable, and is formed of $8a$ surrounded by 11 magnets. It is to be remarked that not alone the tertiaries, but the configurations in the other classes begin with an unstable group of magnets. Thus $8c$ begins the secondaries, $19b$ the tertiaries, $34b$ the quaternaries, and $51b$ the quinaries.

The reader has seen that a given number of magnets may form two or more different configurations. Thus five magnets form two, $5b$ a square with a magnet at its centre, and

5*a* a pentagon. Six magnets give 6*a* and 6*b*. With eight magnets we obtain three configurations, 8*a*, 8*b*, and 8*c*. Now the different configurations formed of the same number of magnets always exhibit different degrees of *stability*. Vibration of the less stable forms (produced by alternately lifting and lowering the superposed magnet) sends them into the stable forms. Thus, 5*b* on vibration rearranges itself into 5*a*, 6*b* into 6*a*, and 8*c* or 8*b* into 8*a*. With the configurations of higher classes (the tertiaries, quaternaries, &c.), even a knock on the table is sufficient to cause the needles of the unstable configuration to move to positions of stable equilibrium.

On looking at the diagrams, it will be observed that only the *stable* primaries form the nuclei of the secondaries; and, moreover, those primaries which are not dimorphous, like 2, 3, 4, and 7, serve as nuclei to more than one secondary. Thus, 2 is the nucleus of 8*a*, 8*b*, 8*c*, 9, and 10*b*; 3 is the nucleus of 10*a* and 11; and 7 is the nucleus to 16, 17, and 18; while each of the other stable and dimorphous primaries, 5*a*, 6*a*, and 8*a*, appears only once as nucleus, respectively to 14, 15, and 18*b*. This same power of the most stable nuclei to resist outside stress is shown in the configurations of the tertiary and quaternary classes; where the secondary 11 appears as nucleus to 21, 22, 23, and 24. The secondary 18*a* persists in even a more marked manner as a nucleus. This 18*a* has the contour of that very stable 7 (the only configuration possible with 7 magnets) which forms its nucleus. Among the tertiaries 18*a* is the nucleus of 30*b*, 31, 32, and 33; while in the quaternaries it forms the inner nucleus of 46, 47, 48, and 49. The fact of the persistence of these stable forms as nuclei may be suggestive to chemists and crystallographers.

It is here to be remarked that (as a general rule holding good in all the classes), of two configurations made up of the same number of magnets, that configuration is the more stable which has the least number of needles for its nucleus.

Illustrations of Molecular Structure. (1) *Unstable Molecular Equilibrium*.—That the molecules in a body may be in a state of unstable equilibrium so delicately balanced that a slight extraneous action of pressure, heat, light, &c. may cause a new molecular arrangement in the body, is shown in many facts. A few of the more familiar ones will answer for our purpose. Thus quiet water, which remains liquid at a temperature of 10° C. or more below 0° C., changes suddenly into ice when agitated; and during this solidification its temperature rises. In like manner a supersaturated solution of disodium sulphate solidifies when a crystal of this substance is dropped into it. Another instance of a sudden change from

an unstable to a stable molecular condition is shown when the yellow crystals of mercuric iodide change, on the touch of a glass rod, to a scarlet colour, with a perceptible motion of their particles. These and similar phenomena are illustrated by the change of unstable to stable configurations caused by vibration, shock, and varying conditions of stress. Thus $5b$ changes into $5a$, $6b$ into $6a$, and $8c$ and $8b$ into $8a$.

(2) *Illustrations of Expansion on Solidification*, as shown by water, bismuth, antimony, cast iron, &c., are readily given by the floating magnets. One volume of water at 0° C. expands, on freezing, into about one and one tenth volume of ice. It happens that the area of $5b$ is greater than the area of $5a$ by about one tenth; so that the increase in area which takes place when the pentagon of $5a$ is changed into the square $5b$ may represent the increase in the volume of water when it changes into ice.

It will be observed, on an examination of the diagrams, that, of two configurations formed of the same number of needles, that configuration which has the larger area has a magnet in its centre. Thus $5b$ exceeds in area $5a$, and $6a$ is of greater area than $6b$. To see the effect of a repulsive centre on a configuration, compare the areas of the two squares 4 and $5b$, and of the two pentagons $5a$ and $6a$. The most marked effect of a repulsive central magnet is seen on comparing 14 with 15. The outside contour of each is formed of 9 magnets. The nucleus of 14 is the peculiar flattened pentagon, which is expanded into symmetry on the addition of another magnet, while at the same time the outside contour of 15 conforms to the regular pentagonal nucleus. These phenomena are so suggestive, that I make bold to put the question, May it not be that there is an actual *centralization* of atoms in the molecule when a body expands in solidifying, and in the case where of two or more isomeric bodies one has always the minimum density? I offer this as a suggestion which may be worthy of the consideration of crystallographers.

(3) *Illustrations of Allotropy and Isomerism*.—The most interesting of our experiments with the floating magnets are those illustrating the phenomena of allotropy and isomerism. It is well known that an elementary substance may exist under very different forms. By the action of heat, electricity, &c. an element may have its physical and chemical properties so changed that no one would suppose that the different bodies thus made out of one and the same element were really all of the same substance. Yet the body remains elementary under the different appearances; for it is impossible by any means of subtraction to get any thing but the elementary substance

from it. Phosphorus, sulphur, and carbon give instances of allotropy. Thus graphite and the diamond are both carbon; yet how different are they! One is soft, opaque, black, and with a metallic lustre; the other is the hardest of bodies, transparent, and resplendent by its refractive action on light. Graphite is a good conductor of electricity, crystallizes in small six-sided tables which belong either to the hexagonal or monoclinic system, and has a specific gravity of 2.2; while the diamond is a bad conductor of electricity, crystallizes in the monometric system, and has a specific gravity of 3.5. Whenever an element or a compound takes two different crystal-forms, these different crystals always differ in their density.

These differences of form and density shown in allotropy and isomerism are well illustrated in the configurations which are formed of the same number of magnets. Take figures 5*a* and 5*b*. The first is a pentagon; the second is a square with a magnet in its centre. The forces in these floating magnets and in the superposed magnet remain the same in all the configurations; and these have all been printed from needles floated in water whose surface was at a constant perpendicular distance from the pole of the superposed magnet. Thus we see how the same atoms, endowed with forces of the same strength, may take different relative positions, and thus produce very different crystal-forms in the same matter. We may take 5*a* for an illustration of the atomic arrangement in the diamond, while 5*b* may stand for graphite. But there is always a change of density accompanying the different forms in allotropy; and this fact is also illustrated by configurations 5*a* and 5*b*. In bodies formed of the same kind of elementary atoms, as in allotropy, it is evident that their relative densities will be directly as the number of atoms contained in the unit of volume. As our configurations illustrating allotropy contain the *same* number of magnets, it follows that the *relative densities* of these configurations are inversely as their areas. Now the area of 5*a* (measured on the original prints) is 818 square millimetres, and the area of 5*b* is 992 square millimetres; hence the density of 5*a* is to the density of 5*b* as 992 is to 818. Thus we see how the arrangement of magnets in 5*a* may stand for the molecular structure in the diamond, while 5*b* may stand for that in graphite.

Numerous instances exist in chemistry of the same elements combined in the same proportions, yet producing bodies crystallizing in different forms, and having different densities, colour, transparency, hardness, &c. As examples of this phenomenon of isomerism we may cite calcium carbonate, which

crystallizes in two forms, differing in density—viz. as calc spar, with a specific gravity of 2.72, and as arragonite, with a specific gravity of 2.93. Configuration 6*a* may stand for the molecular structure of calc spar, while 6*b* may stand for that of arragonite. The relative densities of these two configurations are as 208 to 247.

A striking example of isomerism is given in titanitic acid, which crystallizes in three distinct forms:—as *anatase*, specific gravity 3.82; as *Brookite*, specific gravity 4.02; and as *rutile*, specific gravity 4.25. These three isomers may be illustrated by 8*c*, 8*b*, and 8*a*, which have respectively the densities of 382, 364, and 360.

It will, of course, be understood that the above parallelisms are given merely as *illustrations* of how our experiments may serve to explain and illustrate the phenomena, on the assumption of the atomic hypothesis, and on the supposition that the actions which, in the experiments, take place in a *plane*, may similarly take place among repelling and attracting points situate in space of three dimensions.

Other forms of the Experiments.—Instead of floating the magnets, they may be suspended by fine silk fibres. In this method of experimenting the attractive action of the superposed magnet is replaced by the action of gravity, which draws the mutually repellant needles towards the vertical.

An advantage of this form of the experiment is that the configuration can be transported, and may thus serve in illustration of a moving molecule as set forth in the kinetic theory of gases. It is interesting to watch the mutual actions of two or more approaching configurations, and to observe the motions in the exterior and in the contour of a suspended configuration on its impact against a resisting or a yielding surface.

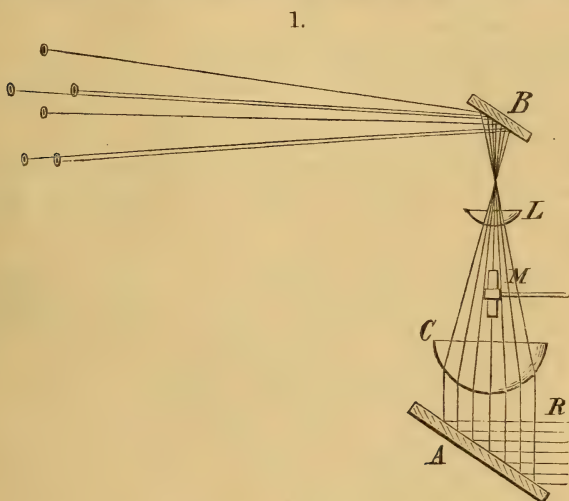
Professor O. N. Rood suggested to me to replace the suspended magnets by gilded pith balls, hung by silk fibres and similarly electrified.

Professor Frederick Guthrie, under date of May 21, writes:—"If the corks are made somewhat wider than in your larger needles, the needles move and arrange themselves very quickly if they are turned over and floated on perfectly pure and freshly filtered mercury. Those which reach the edge incline with their corks in the capillary trough."

Method of projecting the magnified Images of the experiments on a Screen.—To exhibit these experiments before a large audience it is best to use short magnets made as follows:—Magnetize rather large sewing-needles, with their points all of the same polarity; then take each needle between the flat jaws

of a pair of pliers, and with a pair of cutting-pliers snap off the needle close to the jaws of the other pliers. Thus form a series of magnets about $\frac{3}{4}$ inch in length. Run each of these through a thin section of a small cork, and then coat both needle and cork with shellac varnish. Float these magnets in a glass tank placed over the condensing lens of a vertical-lantern; or you may even float them directly on the condenser itself, if this is made of an inverted glass shade filled with water. This form of condenser was first used by Dr. R. M. Ferguson, of Edinburgh.

Figure 1 shows the arrangement of the experiment. The rays of light, R, from a heliostat, or from an oxyhydrogen light,



fall on an inclined mirror, A, placed under the water condenser, C. The needles float on the surface of the water in this condenser. The rays which have passed through the lens, L, are reflected by the swinging mirror, B, to the distant screen, where they form the images of the floating magnets. The magnet is held over the needles at M by means of a wire which is wrapped round the magnet to serve as a handle. If a long magnet be used, it will work well if its pole is brought over the needles* by inclining it.

These experiments with floating magnets give forcible presentations of the reign of law. It is indeed quite impressive to see order being evolved out of chaos as we hold a magnet

* The magnetic needles in the experiments may be replaced by pieces of soft iron wire, which will be magnetized by the induction of the superposed magnet.

over a number of needles carelessly thrown on water, and witness them approaching and, one after the other, entering into the structure of that geometric figure which conforms to the number of magnets composing it.

XIV. *A Condenser of Variable Capacity, and a Total-Reflexion Experiment.* By C. V. BOYS, A.R.S.M., *Lecturer for the Term on Natural Science at Uppingham School**.

WISHING to show my pupils the effect of condensation on the spark, I thought a condenser the capacity of which could be reduced gradually to nothing would be most suitable. So I made this simple contrivance, which answered its purpose well:—

A glass tube is sealed at one end and is covered with tinfoil for one third of its length; this forms the outer coating. The inner coating consists of a test-tube with the rim cut off, also covered with tinfoil; this is fixed to a wire, and can be drawn in and out. When it is fully in, the condenser has its maximum capacity; when drawn out as far as possible, the two coatings are too far apart to have any sensible action, and the capacity is zero.

On hanging this on the conductor of a Holtz machine the effect on the spark is well shown. Let the wire be first pushed in as far as possible, the condenser then acts to its full extent; but on gradually drawing it out the sparks are less and less bright, but follow one another more and more rapidly, till at last, when it is fully out, they have passed gradually to the almost continuous pale spark so characteristic of a Holtz machine. To show the effect best, the poles should not be more than about half an inch apart. Of course much ozone is formed inside the tube.

The total-reflexion experiment was an accident. A small condenser made of a test-tube gave way under the strain, a minute hole being pierced in the bottom, through which sparks passed almost continually. No light could be seen anywhere except on the rim of the tube, which formed a brilliant circle of light. The light from the spark was totally internally reflected in the substance of the tube till it reached the rim, which it struck normally. The bright circle of light (the tube itself being dark) was very striking; and the experiment is a far truer illustration of total internal reflexion than the more beautiful one with a stream of water. The tube is, unfortunately, broken; and I have not succeeded in piercing another with the spark. A crack made with a hot wire does not do so well.

* Communicated by the Physical Society.

XV. *Theory of Voltaic Action.* By J. BROWN*.

SINCE the publication of a former paper on this subject† the apparatus employed in the bimetallic-ring experiment has been much improved by using a finer suspension-wire ($\cdot 001$ inch diameter) for the needle, and electrifying it by means of a Daniell's battery of 100 cells. Each cell is made of a 4-in. \times $\frac{5}{8}$ -in. test-tube, with the copper at the bottom surrounded by copper-sulphate crystals, and connected by gutta-percha-covered wire with the zinc of the next cell. The space between the coppers and zincs is filled with sand saturated with weak solution of zinc sulphate; and zinc filings are mixed with the upper layers of sand to reduce any copper sulphate that might diffuse upwards. The cells are mounted in an ebonite stand. With this arrangement and the copper-iron ring described before†, it is quite easy to get decided deflections of about 3 centims. in air, copper negative to iron. After admission of the hydrogen sulphide the deflections are considerably greater, copper being now the positive metal.

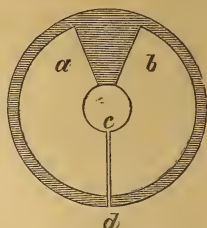
As copper is negative to nickel in water and positive to it in hydrochloric acid, a ring was made of these metals similar in size to the copper-iron one. Here the deflection obtained was about 4 centims. in air, copper negative. Hydrochloric acid gas was caused to flow into the case; and after a few oscillations the negatively electrified needle crossed zero and turned towards copper, the deflection gradually increasing to 1.5 centim. The flow of gas was then stopped; and the deflection slowly decreased. In four hours after, it had fallen to 1 millim. It then began to increase; but the admission of fresh hydrochloric acid gas caused it to *diminish*, a phenomenon the explanation of which is not clear. The reversal of the potential of these metals, on the admission of the gas, however, was quite decided. The experiment was not repeated, as the corroding action of the acid was destructive to the apparatus. The ratio of differences of potential given by these deflections is, of course, rough, as the apparatus is not adapted for exact measurements.

In a simple voltaic cell, consisting of a copper and a zinc plate in connexion and immersed in an oxidizing electrolyte, the current due to the chemical action in the cell flows in the electrolyte from zinc to copper. If, then, we divide the electrolyte by a non-conducting plate, positive electricity ought to accumulate on the side of the plate towards the zinc, and

* Communicated by the Author.

† Phil. Mag. August 1878.

negative on the side towards the copper. In order to prove this, a disk of thin vulcanite, with a hole in its centre and a radial slit, cd , had two paper segments, a and b , fixed on it. These were moistened with water, and the whole placed in the apparatus described before in the same position as one of the bimetallic rings. The needle was set over the slit cd ; and each of the points a and b of the paper segments was in connexion with a slip of moist blotting-paper which passed out of the case. The outer ends of these slips lay side by side on a plate of vulcanite. Touching either one of them with a piece of zinc or copper, or with one end of an insulated copper-zinc pair soldered together, had no appreciable effect on the electrified needle; but when the zinc end of the pair was placed on one slip and the copper end on the other, the index light-spot at once deflected about 10 centims., showing the paper in connexion with zinc positive. When a copper-iron pair was used the iron side was positive; but on placing a drop of potassium sulphide at the junction of the copper and damp paper, the copper side became positive. If the copper-iron pair, instead of being soldered together was joined by a drop of water, no deflection occurred, or only a very small one; but the addition to the connecting water-drop of a small quantity of potassium sulphide caused a vigorous deflection of the needle, the segment in connexion with the copper being now negative on account of the current flowing across the connecting drop from copper to iron.



The slit cd corresponds to the dividing plate in the electrolyte; and if we suppose this plate to be air and its thickness to be increased, replacing the liquid till nothing but a mere film remains on each metal, we have then the conditions of Volta's condenser experiment, where, the plates being close in front of one another and in metallic connexion, the film of condensed moisture &c. on the zinc plate has on its outer surface a positive charge, that on the copper a negative, the layer of air between them preventing the combination of the two electricities. It may be urged that the better the plates of a condenser are ground or fitted together, the more apparent is the contact effect; but it is scarcely to be supposed that we have yet had any experiments with plates so well surfaced that it was certain no air layer was present between them, or so well mounted that they could be kept exactly parallel while being separated, all points on their surfaces separating at the same instant.

In my former paper, the production of a difference of elec-

tric potential by contact of dissimilar substances is attributed to the nature of the gas or atmosphere surrounding them. It is probably so only in so far as such gas produces, by condensation, a film of itself on the surfaces of the substances in question.

In experiments where the difference of potential of a metal and a liquid in contact is the subject of investigation, the arrangement is probably analogous to a two-fluid cell with plates of the same kind of metal, *i. e.* the metal under examination—one of the fluids in the supposed cell being that in contact with the metal, and the other being the film of moisture &c. condensed on the metal.

Slight variations in the nature of this film, due perhaps to the remains of whatever may have been used for cleaning the metal, or to such vapours as might be present in the atmosphere of the laboratory, would produce corresponding variations in the amount of difference of potential observed—just as a small quantity of hydrogen-sulphide in the atmosphere surrounding the copper and iron in contact reverses the relative potentials of these metals. This would no doubt account, at least partially, for the discrepancies in the results obtained by different observers, and indeed by the same observer in different experiments with the same liquid and metal.

Belfast, December 1878.

XVI. *Effect of the Motion of the Air within an Auditorium upon its Acoustic Qualities.* By W. W. JACQUES*.

IT is the purpose of this paper to give an account of some experiments made for the purpose of determining the effects of the currents of air within an auditorium upon its acoustic qualities. These experiments are in three series:—the first being a laboratory investigation into the effects of currents of air upon a ray of sound; the second and third, studies, by different methods, of the effects of the currents of air in a lecture-hall and a theatre, upon the waves of sound.

Since the air of a hall is the medium by which sound is conveyed from the speaker or singer to the hearer, it would certainly seem of fundamental importance that this air should be in the condition best suited to the propagation of sound. Experiments made by the author, in a considerable number of halls, show that the atmosphere is almost invariably disturbed by currents of air of varying density crossing the room in all directions. These currents have been traced out with thistle

* A reprint from the Journal of the Franklin Institute, December 1878, communicated by the Author.

balls, and their velocity measured with the anemometer. The estimates of density have been made from the velocity of motion. Now the experiments of Professor Tyndall have shown that currents of air of varying density form one of the chief obstacles to the propagation of sound-waves. The author, in repeating these experiments in a somewhat modified form, found that such currents of air not only decrease the intensity of a sound-wave, as Professor Tyndall has shown, but that they actually modify its form, and so give rise to great indistinctness. The experiments were made as follows:—At A was placed a source of sound,

A + - + - + - + - + E,

being in some cases an organ-pipe, in others a man who spoke in a clear and distinct voice, and in others various musical instruments on which simple combinations of notes were played. Just below the points + + &c., were placed substances heated to such temperatures as to give rise to currents of air corresponding in density to those found in an auditorium. At E was placed the ear, which, though it be not so reliable an instrument as the singing-flame of Professor Tyndall for estimating intensities of sounds, is, of course, the best instrument imaginable for determining their qualities. The results of the experiments were as follows:—The ear being placed at E, and a small lead organ-pipe, blown with a constant pressure of air, at A, the heated bodies were placed under + +, &c. A very decided decrease in the intensity of sound was noticed; but it was also noticed that the previously clear note lost its *distinctness*. The pipe was removed, and a man was placed at A, who spoke in clear and well-defined tones. The effect was not only to decrease the intensity of his voice, but to make it slightly confused and indistinct, as if each syllable were repeated several times in very close succession. When a flute was substituted for the voice the effect was the same. The effect on a violin seemed to be considerably less. With a drum no effect whatever was observed. The effect seemed to be most marked on the man's voice, or a musical instrument in which the overtones were comparatively small. The explanation of this is very simple. The original ray of sound, striking upon the first current of air, is partially reflected and partially transmitted. The loss of the reflected portion causes a decrease in the intensity of sound. The transmitted portion, striking upon a second current, is likewise divided, and its transmitted portion continues to be so divided as many times as there are variations in the density of the air. Its reflected portion, as well as that of all the succeeding reflections, instead

of being wholly lost, is interrupted in its backward course by the first current of air and reflected along the path of the primary wave, but following it at an interval of time depending upon the thickness of the current of air. Each reflection being thus again and again reflected and divided, we have, following close upon the primary wave, a multitude of secondary waves, which, falling upon the ear, greatly mask the distinctness of the original sound. Currents of air of varying density then cause, first, a *decrease in intensity* of sound, and secondly, an *indistinctness* or confusion of the sound.

That currents of air, which we have thus studied in the laboratory, act in the same way in an auditorium may be shown by the following experiment, in which the sound-waves may be actually traced out in space, and their confusion, consequent upon the introduction of currents of air, likewise shown.

Near the middle of a lecture-hall, 92 feet long and 65 feet wide (the hall of the Massachusetts Institute of Technology in Boston), a heavy plank, 6 feet wide and 12 feet long, was set on one end and firmly fixed. Eight feet from its middle point, on one side, was placed a B 4-stopped lead organ-pipe, which was so connected with a gasometer as to be blown with a constant pressure of air.

On the other side of the plank and within the sound-shadow a system of coordinates, in a plane parallel with the floor, was established by means of light wooden rods, which ran parallel and perpendicular to the board, and their length divided into centimetres.

Now it has been shown by the author (Proc. Amer. Acad. Arts and Sciences, May 10, 1876) that rays of sound diverging from such a source, and being diffracted around the edges of the board, will, when they meet each other, after having passed over paths differing by a half wave-length, neutralize each other and produce comparative silence. By moving a B 4 resonator, connected by means of a rubber tube with the ear, along these coordinates the points of interference are easily detected, and are found to be situated as predicted by formulæ similar to those used in the diffraction of light. In fact the cases are entirely analogous.

We have here, then, a means of mapping out the positions of the sound-waves in space, and can say that at one point there is silence because two sound-waves have met, crest upon the trough, and neutralized; at another the sound is loud, because two waves have met, crest and crest, and trough and trough, and have doubled. All this is true so long as the air of the hall is at rest; and these experiments have been made with doors, windows, and registers carefully closed.

Every thing being in this condition and the sound-waves having been mapped out, the doors and windows were thrown open to the winter's air, and the registers were opened to admit as many streams of air heated to nearly 100° C. What is the effect? Currents are rushing about the hall in every direction (we have the conditions of the laboratory experiment); the waves of sound are superimposed by numberless reflections whose points of condensation do not coincide; and the phenomena of diffraction instantly disappear. This experiment was many times repeated, and always with the same results.

It is evident, then, that in order to procure the proper propagation of sound we must do away with these air-currents. It must be remembered, however, that when large numbers of persons are crowded into halls, the air within is usually subjected to very considerable disturbances in order to obtain even indifferent ventilation. How, then, shall we obtain the desired ventilation and at the same time prevent the formation of currents of air?

The solution of this problem seems to me to be given in the third series of experiments, which were carried out in the Baltimore Academy of Music, designed by Mr. J. Crawford Neilson, architect, of that city.

The ventilation of this house is so arranged as to prevent largely the formation of air-currents of unequal density. According to a survey, made with thistle-balls and the anemometer, of the space contained within the walls of this theatre, the movement of the air is as follows:—

The whole supply of fresh air is admitted at the back of the stage, is there warmed, then crosses the stage horizontally, passes through the proscenium, and then, somewhat diagonally towards the roof, across the auditorium in one grand volume and with gentle motion so as to almost entirely prevent the formation of minor air-currents. It is exhausted partially by an outlet in the roof and partly by numerous registers in the ceilings of the galleries. From this central outlet and from the large flues of the registers the air passes into the ventilating-tower over the great chandelier, which supplies, in its heat, a part of the motive power of the circulation. It is further expelled from the tower by means of large valves so contrived that, while they offer no obstacle to the *egress of air*, they completely *deny it entrance*. The amount of air so passed through the house is, as determined by a series of experiments, about fifteen thousand feet per minute. This amount, sufficient to ventilate the house, is just what seems to be required to impress the proper movement on its atmosphere. That it

is amply sufficient for ventilation is shown by the fact that the thermometers of the upper circle do not vary perceptibly from those of the orchestra circle.

The seating-capacity of the house is about sixteen hundred persons. The acoustics are, if we may judge from the testimony of a large number of singers and speakers, as well as from our own observation, among the best.

The weakest voice is audible to every seat in the house ; sounds such as a sigh, a kiss, or even the simulated breathing of the somnambulist, may be heard in the most distant parts ; and all effects in music are exactly rendered. All singers and speakers agree in describing the facility with which the voice is used on this stage.

It now remains to show that the universally acknowledged acoustics of this house are largely due to the *condition of the air*, and not to the arrangement and material of the walls, together with other well-known causes of success or defect. For this purpose persons have been repeatedly stationed at different parts of the house during a performance, without being informed of the nature of the experiments to be carried out. They have simply been asked to note, at intervals during the evening, the comparative ease with which they could hear the performers. At various intervals during the evening the valves which control the ventilation were reversed, so as to entirely interfere with the unbroken condition of the air and give rise to currents of circulation. Almost invariably the testimony of the hearers would be that, at times corresponding to the interruption of the ventilation, the "sound was dead," was "confused and indistinct;" and it would be observed that people all over the house would make an effort to listen. As an example of these experiments, the following is copied from the author's notes, for the evening of January 24, 1878.

There was a concert, consisting of orchestral music and solos held in the house. At 8 o'clock observers, A, B, C, and D, were stationed respectively in the orchestra, right and left wings of the balcony, and on the bridge that spans the stage above the level of the highest proscenium boxes. They were entirely ignorant of the nature of the experiment, and were simply requested to note the times of good and bad hearing.

At half-past eight o'clock the valves were reversed and remained so till nine, when they were again set aright ; after the performance the following testimony was given by the different observers :—

A. Orchestra—to 9.15, very indistinct ; 9.15 to 10, much better.

B. Right balcony—8.45 to 9.15, sound was dead ; 9.15 to 10, decidedly better.

C. Left balcony—8 to 8.40, good ; 8.40 to 9.15, confused ; 9.15 to 10, good.

D. *Over* stage—8 to 8.30, good ; 8.30 to 9, strong draught, hearing better ; 9.10, draught disappeared.

An examination of this Table shows that the observers in the auditorium found a period of half an hour's duration when the sound was not so plain as it had been before, or was after. This time was of the same length, but from 10 to 15 minutes later than the period of interruption of the ventilation ; but some time is, of course, necessary for the air-currents to form, or, being formed, for them to be destroyed.

The observer (D) over the stage, however, found the hearing *better* during the half hour of interruption ; and this is exactly what would be expected, for the interruption of the current of air through the auditorium causes it instead to rise directly over the stage into the large space in which hangs the scenery, and thence out of the building.

During Neilson's performance of "*Rosalind*," observers A and B were stationed in the first, and C and D in the second balcony from 8 to 10 P.M. At 8.50 o'clock the ventilators were closed and the lobby doors, together with those leading from the lobby to the street, were thrown open.

Thistle-balls let loose from the balconies showed currents of air coming in at the doors and crossing the auditorium towards the stage. At 9.20 the doors were closed and ventilating-valves set aright. The testimony of the observers was as follows :—

A. First balcony—8 to nearly 9, good ; for about an hour, bad ; afterwards, much better.

B. First balcony—8 to 9, good ; 9 to 9.30, bad ; 9.30 to 10, good ; strong current of air felt from the door a little before 9.

C. Second balcony—8 to 8.50, good ; 8.50 to 9.20, bad ; 9.20 to 10, good.

D. Second balcony—8 to about 9, good ; 9 to 9.20, bad ; 9.20 to 10, good.

In the foregoing paper we have studied the effects of air-currents upon the acoustic qualities of an auditorium. That this is only one of the factors on which success depends the author knows full well ; and experiments are now in progress upon some of the other causes which modify the acoustic properties of churches, theatres, and halls.

These, however, have been in part studied before, while the above researches are believed to be entirely new.

XVII. *On the Music of Colour and Visible Motion.*

By Professors JOHN PERRY and W. E. AYRTON*.

[Plates V. & VI.]

AT the present time, when musical instruments of one form or another are employed nearly throughout the whole world, when even the emotions evoked by the sounds of the human voice have given life to the efforts of a whole nation in the 'Marseillaise,' we are apt to forget that our feelings may be excited through other media than sound. But, just as now all kinds of musical instruments are used in rendering the works of great composers, so we may expect that all known methods of exciting emotion will be combined in the grand emotional compositions of the future.

Although our feelings may be worked on through the medium of any of our senses, one only of these has been hitherto cultivated in the highest degree. And the reason of this is, that there exists an infinite number of *easy* ways of producing sound; so that combinations of sound have been used as the vehicle for exciting emotion in us, and in our forefathers, for the last four hundred years; and, as a result, the ear has been slowly trained to act as the conveyer of the varied impressions it is the province of the artist to create, whereas the means in our power of acting through the eyes are even up to the present day clumsy and inadequate.

Of the optical methods hitherto employed to work on the emotions, the oldest is certainly sculpture; but this can never create an emotion unconnected with thought; and the feelings produced by it vary much in different people, and even in the same person at different times. The musical composer, on the other hand, is able to produce a definite succession of emotions which he can vary at will, and which are not utterly different in different people. A piece of sculpture is but suggestive—it merely introduces some simple emotion which acts in a controlling way upon the human mind; so also a fine picture induces a dreamy state and sets one a thinking.

Other emotions, again, are excited in particular people by certain associations connected with a taste, smell, &c.; but these may be likened to the energy stored up in gunpowder or nitroglycerine, to be set free by the smallest accident, whereas the efforts of real emotional art may be likened to those of which the effects may be calculated according to known laws.

For the eye to act as an agent for the emotions, it must

* Abstract of a paper read before the Physical Society, November 23rd, 1878.

receive much cultivation. Even with the experience of sound the ear has gained during the last four hundred years, how very few people are there sufficiently educated to have their feelings excited through music in answer to the emotions of a composer? and how pleasing to all is the *repetition* of a strain in a melody or the movement in a dance, perhaps from the instantaneous education the ear or the eye receives which enables it to better understand the movement when repeated. Consequently, if we consider the cumbrous and expensive nature of the apparatus necessary for producing regular changes of colour, or of motion, or of the size of the moving bodies, we may expect that it will probably take a long time before the world is able to employ our more complicated agencies.

It may appear at first sight that in placing motion on a footing of equality with what *we* consider its sister graces—sculpture, painting, and music,—we offered an indignity to these latter; and it may appear inconceivable to many how any amount of study of moving bodies can ever create an art as powerful and as enchanting as music. But it must be borne in mind that our present form of the fine arts probably only owes its existence to the accident that western nations have more assiduously educated the emotional side of their minds in certain particular directions*.

And in our own country we have a close connexion between the varied emotions created by colour or movement and those excited by sound. It is well known that when certain persons hear an Oratorio, an Opera, or even a well-played violin solo, they see, without any voluntary effort on their part, beautiful changing mosaics, the patterns of which have definite connexions with the musical chords, and that such people always see a flash of light when they hear a sudden shriek of a railway-whistle.

The emotions excited by large bodies having a great velocity do not seem to be producible by any thing else in nature. These are felt when we stand on a bridge over a railway when a train approaches and passes underneath at great speed, or when we stand at the side of a railway when the train passes, even if we hear no sound, or if there is no appreciable trembling of the earth. In our first experience of such visible motion, terror, due perhaps to its utter strangeness, predomi-

* Here followed a number of examples taken from the Japanese stage and musical performances, proving that great conventionality existed among different nations in the expression of the emotions, and lending weight to the doctrine that music, unlike painting, received no suggestion from nature, and was therefore a creation of each individual people.

nates. Similarly, the emotions produced by the play of colours in a fine sunset, by the rolling of the waves on the sea-shore, by the rhythmic motion of a large engine or of a long pendulum, by the tracing out of the combined harmonic curves, which have now become well known to all who have heard lectures on vibrating bodies, seem to be quite different from one another, and from the emotions produced by sound.

We have therefore tried to make a machine which to the emotions produced by a combination of colour, a mass in motion, and by motion in curved paths, would bear the same relationship as a musical instrument to music. We have not ventured to give our machine a name, since the name of the instrument should be that of the art; and although we have a name for exciting emotion by sound (music), we lack names both for the art of exciting emotion by colour, and by moving bodies.

Many instruments have already been devised for combining together two harmonic motions; but as the conception of using such machines as emotion-excitors has never been present in the designers' minds, the performances of these instruments, although very beautiful, are necessarily of a comparatively elementary nature. The most important of these machines, all of which, with the exception of one or two Prof. Guthrie has been so very kind as to have arranged in working order before us, are Blackburn's pendulum, Wheatstone's kaleidophone, Lissajous' tuning-forks, Yeates's vibrating prisms, Donkin's harmonograph, Tisley's and Spiller's harmonograph, and Hopkins's electric diapason. In some of these, as in Blackburn's pendulum, only *one* particular pair of harmonic vibrations can be combined, and any change in the period of either means a stoppage of the instrument, corresponding in music with a delay in the tune at the end of every chord; in others we can change the period of one or other of the component harmonic vibrations, but have no certain means of controlling the amplitude, which in music would be equivalent to an inability to render at will a note *forte* or *piano*; or, rather, as it is not only the strength of the entire note, but even the amplitude of the various component harmonics that these instruments cannot regulate, it would be as if in music there was the probability of a note marked in the score as *piano* for the flute being rendered by a loud blast on the trumpet. In only one of these harmonic-motion compounders with which we are acquainted, viz. in the most perfect of the existing ones, Tisley's harmonograph, can an elliptic and a linear motion be combined; but even in this case a change from this motion to any other can only be made by first stopping the machine; and in none

of the machines can a sudden change be produced in phase. Now it is obvious that we require a motion-producer of far greater range than any of these, if we are to play on the emotions, since all the qualities—elation and depression, velocity, intensity, variety, and form (which it is considered are possessed by a complex emotion)—must be visibly rendered.

The result produced by our instrument is this:—A round shadow is thrown upon the plain white wall of the room in which the audience are seated. The shadow appears to be a large black ball, of which the size during the performance may be made to vary. It has motions over the wall which are pure harmonic or combinations of harmonic motion, produced by our being able to give to the shadow two independent motions—one in a vertical line, the other in a horizontal line, *each* consisting of a *combination* of linear harmonic motions, the *amplitude, period, or phase* of any one of which may be varied at will. We give a collection in figures (A to U, Plate V.) of some of the most simple paths traced out by our moving ball, and which differ from the ordinary Lissajous' figures in that we have placed little circles at such distances asunder as are passed over in *equal times*, in order to give an idea of how the velocity varies at different parts of any such path. Unfortunately, however, for giving a conception of the appearance, it is this variation of velocity, the effect of which on the senses no figure or description can give any idea, which constitutes one of the most striking features of the exhibition. Many of our readers will, however, have seen the motions of a very large Blackburn's pendulum, and of a very long simple pendulum; and they will therefore have gained such an idea of the motions of which we speak as a person, who has heard only the chirp of a sparrow, has about Beethoven's sonatas.

It must be remembered that while all the possible paths of the moving body are beautiful in shape, they are also endless in their variety. Not only may they vary without limit in their form, but also in their size, and in the velocity with which the body moves along them. Thus, perhaps the body is swinging slowly in a straight line in any direction, like the swaying of a huge tree in the wind, or so rapidly that a dark line only is visible, when, touching a key, the line is suddenly seen to open out, and the body rolls round a small or great circle, or a small or great ellipse of any proportions and with any velocity*; or the figure may be like any of the simple figures

* One of the most beautiful things in connexion with the recent monster captive balloon in Paris was its rolling round and round in the breeze, like a huge inverted conical pendulum, after the stay ropes had been liberated, just before its ascent.

illustrated in the diagrams, or any others of the millions of different and more complicated forms producible. Some may change all their dimensions gradually in a certain direction, while dimensions in the other directions remain unchanged; others may alter all their dimensions in different directions simultaneously, but with different velocities. Now, as we gaze at the body with all these graceful complex motions, which can readily be varied in shape or size of path, or in velocity of description, the same sort of awe comes over us as must have been felt by the people when they first listened to the strains of the earliest musical instrument.

To be sure it can never be as easy to change from a grave circular swing to a quick and complicated motion as it is to put down the key of a pianoforte; and it may be long before mechanicians will be able to let us vary the motion with sufficient rapidity. But we found that we could, even with our imperfect instrument, change one form of motion to almost any other possible one in about a second; and this is sufficiently quick for the present educational state of the art.

Our instrument itself we unfortunately cannot show, as it is the property of the Japanese Government, and is in Japan. Some photographs, however, of it and of our assistants who took part in its construction and performances are lying on the table. The exhibitions have been confined to ourselves and our students; but it was our intention, after we had educated ourselves by practising with the machine, to exhibit it publicly. Unfortunately, however, from various delays connected with improvements and alterations necessarily connected with the designing of such a new machine, it was hardly completed before one of the present writers left Japan, and the other has now no opportunities to practise with it. But it is our intention to construct a new instrument with many improvements on the first model, one of the most important being the carrying out of the idea we had at the commencement, of enabling the operator, by touching keys, to give any desired brilliant or sombre coloration to the wall on which the shadow of the body is moving, or to play on the wall a changing mosaic.

We will now describe the simplest form of our instrument, represented in fig. 1 (Plate VI.) about one sixteenth of its full size.

B C is a roller which is turned by a handle D (not visible in the figure), the fly-wheel E being of use in steadying the motion. The roller is divided into three portions, B F, F G, G C, by the circular collars at F and G; at H, J, K, the centres of these portions, the section of the roller is circular,

whereas at any other place it is such that, acting like a cam or tappet, it gives pure harmonic motion to a slider kept pressing on the roller, and only capable of radial motion. Such a sliding piece, resting any where between K and C, receives one complete harmonic motion during one revolution of the roller; if it rests anywhere between G and K, it completes two pure harmonic motions during a revolution, three between J and G, four between F and J, five between H and F, and six between B and H. The amplitude gradually decreases as the sliding piece is made to rest on places nearer the circular section, where of course there is no up and down motion of the sliding piece.

Every section, therefore, of the roller has a curved outline, of which the construction is easy. Thus suppose we want the section which will give a slider five complete harmonic swings in one revolution of the roller. Make the angle A O B (fig. 2) equal to one fifth of four right angles; describe the circular arc A B with O as centre and radius O A equal to $R+r$, where R is the radius of each of the three circular parts of the roller H, J, K, and r the radius of the small friction-wheel on the end of the sliding piece; make B D equal to half the maximum swing we wish the slider to receive. Then with centre B and radius B D describe the semicircle 159, which divide into any number of equal parts (eight in the figure), and let fall perpendiculars 22, 33, 44, &c. from the points of subdivision on to the diameter D B 9, meeting it in the numbered points. With O as centre, describe a circular arc through each of these numbered points in D B. Divide the angle A O B into twice the number of equal parts into which the semicircle 159 was divided. Then draw a curve 1 C D through the intersections of the first arc and first radius, the second arc and second radius, &c. Finally, draw a great many equal circles with radius r , the centres being in the curve; then the envelope D E F (fig. 3) is one fifth of the whole curved section we wish the curved roller to possess; and a template of tin may be made to be used in the construction of the roller. It will be found advisable to construct four templates for each division of the roller, since, although a section of the harmonic surface described by the *centre* of the little friction-wheel formed by a plane passing through the axis of the large roller in a straight line, such a section of the actual surface on which the little wheel rests will be curved in consequence of r , its radius, not being infinitely small. Our large roller was made of hard wood; but it would have been much better if it had been made of cast iron or steel, since when of wood it, as well as the little friction-wheel of the sliders, must be made large to avoid abrasion; but even when these are large it is

very difficult to avoid abrasion, and consequent slight irregularities in the motions of the slider, since the springs pressing down these sliders must be moderately strong to cause them to promptly follow all the alterations in curvature of the different parts of the roller.

Fig. 5 shows (reduced to a scale of one sixth) the roller as it came from the lathe before being cut to fit the templates; and figs. 6, 7, 8, 9, 10, 11 the sections at AB, CD, EF, GH, IJ, KL (fig. 5) one quarter full size. Small circles have been drawn representing the little friction-wheel, and at such distances apart that in all cases the time taken for the wheel to pass from one position to the next is constant and equal to the forty-eighth part of the period of the revolution of the roller.

We used six sliders, α , β , γ , ϵ , ζ , η (fig. 1), one of which is shown enlarged in fig. 4. Each of these sliders could be moved longitudinally, parallel to the axis of the roller, along two stout iron bars LM, OP (fig. 1), in order to alter the amplitude of the swings. Sliders α and ϵ could be made to rest anywhere between B and F, β and ζ anywhere between F and G, and γ and η anywhere between G and C. Each slider carried at its upper end a large pulley made to move very easily (fig. 4); and the three pulleys of α , β , γ were always in the plane of the fixed pulleys T, U, V attached above to the wooden frame. A fine inextensible cord passing round the movable pulleys α , β , γ , and the fixed pulleys, S, T, U, V, was fixed at one extremity to the pulley V (the purpose of which was simply to adjust the length of the cord); and at the other end, where it hung vertically, it was attached to the top of the glass plate *abcd*, from the bottom of which hung a weight in a pail of water to damp the motion of the weight. This system then gave to the glass plate the sum of the motions of the sliders α , β , and γ . A similar cord over ϵ , ζ , η and W, X, Y, Z, together with the cord passing over the fixed pulley Q, and to which also hangs a weight in a pail of water, gives to the glass plate a horizontal motion equal to the sum of the harmonic motions of the sliders ϵ , ζ , η . It is evident, therefore, that to a circular patch stuck on the centre of the glass we were able to give motions compounded of the above harmonic motions perpendicular to one another, and by projection, by means of an electric light or heliostat, to cause this motion to appear like that of a large black ball rolling about on a white or coloured background.

Even still greater variety could have been imparted to the figures by the metal rods LM, OP (on which the sliders moved) having a motion at right angles to the radius of the roller. This could easily be arranged if the ends of the metal

rods moved in circular grooves ; and the result would be that not only could we alter the amplitude of any one of the component harmonic vibrations by moving a slider along the rod, but we could also alter the phase by giving the rod a circular motion. To do this, however, satisfactorily would have required either the employment of a much larger roller, so that the slowest vibration of a slider occurred four or five times during one revolution of the roller, or else a change in the arrangement. In the illustration (fig. 1) the glass plate, for simplicity is shown merely kept in position by the four cords ; but in reality it moved in a horizontal frame which again slid in a vertical one, so that any lateral motion at right angles to the plane of the glass was impossible.

The reason why there is necessarily a considerable distance between the sets of fixed and moved pulleys is in order that a longitudinal motion of the slider shall not alter the *mean* horizontal or vertical position of the spot on the glass. At first we had the cords much longer than shown in the figure ; but then, even after great care had been taken in endeavouring to obtain inextensible cords, some stretching was found to take place in practice ; consequently we were compelled to determine experimentally what was the greatest length of cord that could be used without the stretching interfering with the accuracy of the motion ; and this length was the one employed in the actual apparatus.

The ingenious way in which a number of pulleys are made to give the sum of their motions to the extremity of a cord was suggested to us by the arrangement employed in Sir W. Thomson's tide-calculating machine ; but it is possible that in our new machine we shall adopt a totally different plan, and one which we think is new. If the two extremities of a long rigid rod have parallel motions perpendicular to the rod, the middle of the rod has a motion equal to half the sum of the extremities. Thus the parallel motions of two, four, or 2^n points may be compounded. Similarly for the three points, one third of the sum of parallel motions is obtained from the centre of a rigid triangular piece of which the points are the corners ; so that by bars and frames of simple construction it is easy to get the sum of the parallel motions of any number of pieces.

We think the roller-plane has much to recommend it ; but a series of little cranks may be better. Let a number of parallel shafts, having their bearings near their ends on two sides of a trough-shaped metal frame, receive from a system of spur-wheels near their centres relative velocities 1, 2, 3, &c. Let A (fig. 13) be either end of any shaft, B being the corresponding bearing ; and let there be attached to it, and

therefore revolving with it, a sort of double wheel of metal of the form shown. $I I$ is the pulley round which the cord passes, if it is by means of a cord that we sum the harmonic motions ; or the pulley may be absent ; and the axle of the pulley is then simply a crank-pin. Then the rate of revolution of the shaft A determines the period, the distance of the axle $F G$ from the shaft the amplitude, and the position of $F G$ relative to a fixed diameter $K L$ the epoch of the component harmonic motion. By shifting, then, the axle $F G$ we may alter the epoch for any multiple of $\frac{\pi}{4}$ for the greater amplitudes, and of $\frac{\pi}{2}$ for the smaller. As there is, however, only a limited change of epoch, we think, on the whole, that our improved roller method is to be preferred.

We have used combinations of pure harmonic motions for obvious reasons ; but it is possible that, instead of each section of the roller giving a pure harmonic motion, it may be found more suitable to have it giving some other kind of periodic motion ; and such an instrument will differ from the preceding in pretty much the same way that one musical instrument differs from another. As various means have already been devised, by revolving sheets of parti-coloured glass, for producing the effects of the chromotropes of the magic lantern, which physiologists have informed us produce such marked and instantaneous effects on the nervous constitution, and physical organs, of children, we have not yet specially turned our attention to the mechanical details of the colour portion of our machine.

In what has preceded we have spoken only of projecting the motion of a single ball on a wall ; but there is no reason why the motions of several balls should not be gazed at simultaneously, nor why the people of a large city should not have an exhibition of the colour and motion art upon a canopy of clouds on a dark night.

For assistance rendered us during the construction of this apparatus, and for the general intelligent interpretation of our wishes, we have to thank our late assistant, Mr. Kawaguchi, one of the brightest of the students of the Imperial College of Engineering, and one whose constant earnestness of purpose, while it rendered his life the more valuable to the scientific development of Japan, now makes his recent death the more to be deplored.

XVIII. *On Catalysis, and the Nomenclature of Oxides.* By THOMAS BAYLEY, *Assoc. R.C.Sc.I., Demonstrator of Practical Chemistry, Analysis, and Assaying in the Mining School, Bristol**.

AMONG the most remarkable, and at first sight the most inexplicable, of the heterogeneous class of reactions known as catalyses are the decompositions undergone by hydrogen peroxide in contact with certain substances. But on further examination these phenomena afford us the means of explaining and differentiating a considerable number of similar reactions and of effecting a classification of them. As is well known, hydrogen peroxide, although one of the least stable of well-defined compounds when in the concentrated form, becomes when diluted, especially in the presence of acids, much less subject to decomposition.

A solution of peroxide of hydrogen, sufficiently dilute to remain unaffected for a considerable time when mixed with dilute solution of sodic hydrate, may be used effectively in the preparation of the peroxides of several metals. If to a solution of a cobalt salt, sodic hydrate and then hydrogen peroxide be added in the cold, the hydrate of cobalt at first precipitated becomes rapidly blackened, and a hydrated peroxide of variable composition is produced. Almost immediately a rapid evolution of oxygen takes place; and before long the whole of the excess of peroxide of hydrogen is decomposed, the cobalt peroxide, however, remaining unchanged. Under similar circumstances salts of lead and manganese give the same reaction, namely the formation of a dark-coloured peroxide and the decomposition of the excess of hydrogen peroxide. With copper the effects are somewhat different: on the addition of the hydroxyl the suspended blue hydrate of copper assumes a transient yellowish-red colour, but almost immediately resumes its original appearance, while the evolution of oxygen proceeds as in the former cases. Nickel, although possessing properties remarkably resembling those of cobalt, under the influence of the mixture of soda and hydroxyl is not peroxidized; the suspended green hydrate is permanent, and the liquid retains for a long time the properties of a solution of hydrogen peroxide. But although the latter has not the power of peroxidizing nickel salts, the hydrated peroxide of nickel, formed by other means, effects almost as readily as does the corresponding cobalt compound the destruction of hydroxyl. It is this difference between the behaviour of nickel and cobalt that affords the key to these remarkable phenomena. It is evident, and it

* Communicated by the Author.

is proved by experiment, that a small quantity of cobalt peroxide can effect the decomposition of an unlimited amount of hydroxyl, time being, of course, an important factor. On the other hand, the amount of hydroxyl destroyed by the nickel compound is simply that required for its own decomposition.

The causes of the resolution of hydrogen peroxide into water and oxygen by contact with these peroxides may be due, not to any opposite polarity of the two oxygen atoms, contained one in the metallic oxide and the other in the hydroxyl, which coalesce to yield a molecule of oxygen, but simply to the presence of a *strain* in the two reacting molecules. These *oxides*, water and cobalt or nickel oxide, are compounds in which the attractions are, as it were, evenly balanced, the attraction of the metal, or hydrogen, being satisfied by the counteracting atom of oxygen; and the fact that special means, such as the employment of powerful oxidizers, must be taken to introduce additional oxygen into the molecule, justifies the view that the latter is held there by a comparatively weak affinity—and that the condition of this extra oxygen is such, that when in the presence of a foreign body similarly constituted, for example hydroxyl, there results an effective outward strain. The affinity between the extra atoms of oxygen in the two compounds being greatest, they coalesce to form oxygen, leaving the metallic oxide and water. Cobalt, however, has a greater affinity for oxygen than has nickel; and while the cobalt oxide thus formed becomes immediately reoxidized by the excess of hydroxyl, nickel once reduced is not further acted upon. This difference in the deportment of the two metals confirms the view that catalysis is due to a series of molecular decompositions and reformations of the catalyzing body.

The mutual decomposition of hydroxyl and silver oxide closely resembles that between nickel peroxide and the same body. For although the silver compound is not a *peroxide* in the ordinary sense, the ease with which it is reduced by heat and by comparatively weak reducing agents shows that its oxygen is held by a weak attraction; and when it is in contact with hydroxyl, there is a tendency for the oxygen in the silver oxide and the second loosely combined oxygen atom in the hydroxyl to unite. The metallic silver thus produced is not reoxidized; and the action is therefore not continuous. When hydroxyl acts upon suspended copper hydrate, the first product is a yellowish-red substance, probably a peroxide, of transient existence, which disappears as soon as the catalysis commences, and is never afterwards visible. The action, although differing from that of cobalt in that there is no final production of a peroxide, resembles it in the essential fact of

consisting of a series of decompositions and reformations. It is therefore a catalysis. Hydrated sesquioxide of iron and hydrate of zinc have no action upon dilute hydrogen peroxide. We may therefore argue from this, as from its other properties, that sesquioxide of iron is not a *peroxide* in the sense in which the term is used in this paper, namely as denoting an oxide in which one or more of the atoms of oxygen are held in such a way as to have an outward strain in presence of hydroxyl or a similar body. This definition being accepted, the sesquioxide of iron must be regarded as the *oxide* of iron, the term protoxide being reserved for the lower compound. In the same way we should speak of Ni_3O_5 as a *peroxide* of nickel, of NiO as the oxide, and of some lower, at present unknown, compound as the protoxide. Our view of catalysis being as above stated, it is easy to explain the action of nickel or cobalt upon hypochlorites. These are capable of peroxidizing nickel; and this constitutes the difference between the action of the latter with them and with hydroxyl. The cycle of operations can be performed and catalysis established. A further explanation of the difference between the action of copper and of cobalt with hydroxyl suggests itself. The change constituting the cycle in the case of copper is from hydrated oxide to an unknown peroxide and back again; and since the final moment of catalysis, that in which the last hydroxyl disappears, must consist in the reduction of the copper to the lower of the two limits between which the action alternates, we find oxide of copper and water as the final products. In the catalysis of hypochlorites by cobalt, the two limits are probably tricobaltic pentoxide (Co_3O_5) on one side, and cobaltic dioxide (Co_2O_3) on the other; since (Co_3O_5) has been shown to be the result of the reaction.

The decompositions of dilute hydroxyl are very distinct from those which occur to the concentrated compound on contact with finely divided silver, gold, or platinum, with various oxides (as, for instance, with nickel oxide), and with many other bodies. We cannot suppose that finely divided gold or platinum is acted upon by hydroxyl; the action is probably connected with some power possessed by the surface, and especially by edges and by fine particles, of these metals to attract oxygen. In concentrated hydroxyl at ordinary temperatures the oxygen is quivering on the verge of liberation, and such a surface action is sufficient to induce decomposition.

The phenomena of catalysis thus group themselves into physical and chemical. Physical catalyses are those in which the catalyzing body, often an inert substance chemically, remains unchanged and exerts a surface action purely phy-

sical. Chemical catalyses are those which consist of a true chemical action; and are distinct from ordinary chemical actions only in this, that one of the bodies remains in the same state after the action as it was before it.

According to this view of catalysis and of the constitution of oxides, we retain the old terms *protoxide*, *oxide*, and *peroxide* (distinct from acid anhydride) with somewhat different meanings. Oxide of copper (CuO) is no longer the protoxide but the oxide; the unknown yellow body is the hydrated peroxide, while Cu_2O is the protoxide. In like manner the oxygen atoms which in peroxides are held with the least affinity may be called the *peroxygen* atoms, and those which are the last to leave the compound may be called the *protoxygen* atoms. This nomenclature is distinct from the ordinary one founded upon the use of the suffixes "ic" and "ous," and of numerical prefixes, and need in no way interfere with it. Being founded strictly upon the chemical behaviour of the compounds and not upon the constitution of their molecules, it possesses evident advantages; but the other terms have important uses, and might with advantage be retained in the majority of instances.

XIX. *Crystallography of the Nitrosoterpenes of Dr. Tilden.*

By N. S. MASKELYNE, F.R.S.*

[Plate VII. figs. 1-6.]

THE varieties of nitrosoterpenes obtained in crystals by Dr. Tilden belong to two crystalline types. The first includes the substances formed (*a*) from ordinary turpentine, (*b*) from French turpentine, (*c*) from juniper turpentine. To the second type belong the substances obtained from the oils of orange, of bergamot, and of caraway.

I. *First group*.—The crystals of nitrosoterpene produced in different ways from the American oil of turpentine have already been described in connexion with Dr. Tilden's notice of the substances in the Journal of the Chemical Society, June 1875. They were of two kinds, differing in habit—the one being twinned on the plane 001, and the other not evincing any twin habit. The crystals obtained from French oil of turpentine and from juniper oil are very dissimilar in appearance to those made from the American oil; but a goniometrical study of them proves that they belong to the same crystalline type with those previously investigated. The crystals of the latter kinds furnished me by Dr. Tilden presented considerable difficulty under measurement, since certain of the faces are rounded; and from their being very minute and in-

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clined at small normal angles on each other, it is often difficult to discriminate the faces lying in a particular zone, as the greater number of the determinations have to be made with faces which offer no images or such as are very confused, and which can be measured only by the method of maximum illumination.

The figures 1 and 2 represent the same forms as those already given in the Journal of the Chemical Society. Fig. 3 exhibits the forms which exist on the crystals obtained from the French and the juniper oils, together with some other forms, of which the existence on some of the crystals is probable, though no reliable measurements could be obtained from them. The crystals rapidly lose by exposure such lustre as they have when fresh—apparently in consequence of evaporation taking place at ordinary temperatures, as evinced by a faint odour perceptible even while a little crystal is exposed on the goniometer.

The measurements obtained from crystals of the different sorts are arranged in columns,—the first column consisting of the calculated angles as given in the earlier communication to the Chemical Society; column II. containing the angles obtained by measurement of the crystals thus described, which were obtained from American and ordinary oil of turpentine. Columns III. and IV. give the averages of the angles (omitting the extremes) as obtained by measurements from the crystals made from French and from juniper turpentine respectively; while column V. gives the angles calculated on average data, obtained from what seemed to be the better measurements, yielded by the two latter varieties of the crystal. The forms on these “French” and “juniper” nitrosoterpenes were by no means uniformly the same. Some of the crystals were very complicated, exhibiting numerous forms of which even the zone-positions could be only approximately ascertained; others were much simpler, the faces of the forms $\{111\}$, $\{201\}$, $\{110\}$, in one case 031 apparently, with traces of 001 , forming the combination. On others the form u or $\{312\}$ seemed the most important face. The faces of the form $\{110\}$ are in the latter varieties always corrugated by a series of planes inclined at from 1° to 3° on each other, approximately in the zone $[110, 001]$. The following letters represent the faces and forms of the crystals:—

$a, \{100\}$; $m, (110)$; $b, \{010\}$; $m', (\bar{1}10)$; $\bar{m}', (\bar{1}10)$;
 $k, \{101\}$; $\delta, \{201\}$; $c, \{001\}$; $h, \{011\}$; $n, \{031\}$;
 $p, \{111\}$; $t, \{112\}$; $y, \{332\}$; $u, \{312\}$; $\rho, \{512\}$;
 $\lambda, \{\bar{3}51\}$.

	I. Calcula- tion.	II. Ame- rican.	III. French.	IV. Juniper.	V. Calculation.
$\left[\begin{array}{l} a m \\ m b \\ m m' \\ m \bar{m}' \end{array} \right]$	$\begin{array}{l} 53^{\circ} 4' \\ 36 56 \\ 73 52 \\ 106 8 \end{array}$	$\begin{array}{l} 53^{\circ} 4' \\ 36 56 \\ 73 52 \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ 74 19 \\ 105 55 \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ 75^{\circ} \text{ to } 75^{\circ} 19' \\ 105 18 \end{array}$	$\begin{array}{l} 52^{\circ} 50' \\ 37 10 \\ 74 20 \\ 105 40 \end{array}$
$\left[\begin{array}{l} a c \\ a k \\ a \delta \\ c a' \end{array} \right]$	$\begin{array}{l} 70 17\frac{1}{2} \\ 43 44\frac{1}{2} \\ \text{.....} \\ 109 42\frac{1}{2} \end{array}$	$\begin{array}{l} 70 19 \\ \text{.....} \\ \text{.....} \\ 109 40 \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} 70 23 \\ 43 38 \\ 29 52 \\ \text{.....} \end{array}$
$\left[\begin{array}{l} m c \\ c m' \\ m p \\ p c \\ m y \end{array} \right]$	$\begin{array}{l} 78 18 \\ 101 42 \\ 37 26 \\ 40 52 \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} 78 18 \\ 101 42 \\ 37 25 \\ 40 38 \\ 30 8 \end{array}$	$\begin{array}{l} 78^{\circ} 12\frac{1}{2}' \mid 21\frac{1}{2}' \\ 101 47\frac{1}{2} \\ 37 55 \\ 40 45 \\ 29 31 \end{array}$	$\begin{array}{l} 78 18 \\ 101 42 \\ 37 22 \\ 40 56 \\ 30 3 \end{array}$
$\left[\begin{array}{l} m \delta \\ m' \delta \\ \delta u \\ m' p \\ m' u \\ m u' \end{array} \right]$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \\ 86 20 \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} 58 35 \\ 120 45 \\ 17 4 \\ 86^{\circ} \text{ to } 87^{\circ} 15' \\ 75^{\circ} 7' \text{ to } 39' \\ \text{approx.} \end{array}$	$\begin{array}{l} 58 27 \\ \text{.....} \\ \text{.....} \\ 87 9 \\ 75 0 \end{array}$	$\begin{array}{l} 58 25 \\ 121 35 \\ 16 39 \\ 86 51\frac{1}{2} \\ 104 56\frac{1}{2} \\ 75 4 \end{array}$
$\left[\begin{array}{l} m' n \\ p' m \\ m' \lambda \end{array} \right]$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ 93^{\circ} \text{ approx.} \\ \text{.....} \end{array}$	$\begin{array}{l} 53 55 \\ \text{.....} \\ 19^{\circ} 40' \text{ to } 20^{\circ} 28' \end{array}$	$\begin{array}{l} 54 2\frac{1}{2} \\ 93 8\frac{1}{2} \\ 20 3 \end{array}$
$\left[\begin{array}{l} b p \\ p p' \end{array} \right]$	$\begin{array}{l} 57 43 \\ 64 34 \end{array}$	$\begin{array}{l} 57 41 \\ 64 35 \end{array}$	$\begin{array}{l} \text{.....} \end{array}$	$\begin{array}{l} \text{about } 63^{\circ} 35' \\ 64 30 \end{array}$	$\begin{array}{l} 64 26 \\ 64 4 \end{array}$
$\left[\begin{array}{l} m k \\ m u \\ u m' \\ m' t \\ t' m \\ h m' \end{array} \right]$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ 46^{\circ} 30' \text{ to } 48^{\circ} 8' \\ 131 \text{ approx.} \\ 92 30 \\ 87 17 \end{array}$	$\begin{array}{l} \text{.....} \\ 46^{\circ} 55' \mid 47^{\circ} 32' \\ \text{.....} \\ 91 40 \\ 87 22 \\ 67^{\circ} 30' \text{ to } 70^{\circ} \end{array}$	$\begin{array}{l} \text{.....} \\ 47 12 \\ 132 48 \\ 92 40\frac{1}{2} \\ 87 19\frac{1}{2} \\ 68 32\frac{1}{2} \end{array}$
$\left[\begin{array}{l} u u' \\ t t' \end{array} \right]$	$\begin{array}{l} \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{.....} \\ \text{.....} \end{array}$	$\begin{array}{l} \text{about } 28^{\circ} \\ \text{.....} \end{array}$	$\begin{array}{l} 28^{\circ} 40' \text{ approx.} \\ \text{.....} \end{array}$	$\begin{array}{l} 29 48 \\ 40 59 \end{array}$
$m(c)\bar{m}'$	23 24	23 25		
$a p$	52 20	52 28		

A facile cleavage runs parallel to the face (001); a less facile cleavage is parallel to the faces of the form {110}. The great facility with which the former cleavage is produced precluded the forming sections for the polariscope.

In one crystal which showed one of the ring-systems the plane of the optic axes was evidently perpendicular to the plane of symmetry, the acute bisector lying in that plane; and it appeared that the dispersion of the bisectors in that plane for different colours gave the position of the bisector for blue rays nearer to the normal of a , or (100), than that of the red

rays. The position of the acute bisectors was approximately determined as being about $11\frac{1}{2}^\circ$ on the normal to (100), and $32\frac{1}{4}^\circ$ on the normal to (101).

II. *Second group.*—The second type of the crystals of nitrosoterpene (figs. 4 & 5) includes those formed from the oils of orange, bergamot, and caraway. They belong to the mono-symmetric system; and their arc-elements may be represented as $100\ 001 = 79^\circ 1'$, $100\ 101 = 38^\circ 25\frac{3}{4}'$, $100\ 110 = 40^\circ 25\frac{1}{2}'$. The crystals representing the orange and the bergamot preparations, especially the former, generally give fairly good images from the faces 001, $\bar{1}01$, and 110; those from 100 are usually in the condition of many bands; while the reflections are never good from the face 101. The faces {001} are generally hollowed. The reflections from the faces on the caraway crystals, and particularly from 100, are less perfect than those from the other preparations. In the following Table the measurements marked with an S were made by Mr. Elliot Steel. The only faces that occur on any of these crystals are those of the following forms:—

a , {100}; c , {001}; k , {101}; d , $\{\bar{1}01\}$; m , {110}

	Calculated.	Bergamot.	Orange.	Caraway.
$a\ c$	$79^\circ\ 1'$	$79^\circ\ 2\frac{1}{2}' \mid 79^\circ\ S.$	79°	79°
$a\ k$	$38\ 25\frac{3}{4}$	$37\ 14$	$38\ \text{to}\ 40^\circ$
$c\ d$	$24\ 4\frac{1}{2}$	$24\ 4 \mid 24^\circ\ 6' S.$	$24\ 4 \ \&\ 24\ 6\frac{1}{2}$	$24\ 8$
$d\ a'$	$76\ 54\frac{1}{2}$	$77\ 2 \mid 76\ 34 S.$	$76\ 56$	$77\ 21$
$a\ d$	$103\ 5\frac{1}{2}$	$103\ 3\frac{1}{2}$	$103\ 5$
$m\ m'$	$99\ 9$	$99\ 7 \mid 99\ 10 S.$	$99\ 9$	$99\ 8$
$m\ \bar{m}'$	$80\ 51$	$80\ 52$	$80\ 52$
$a\ m$	$40\ 25\frac{1}{2}$	$40\ 26\frac{1}{2} \mid 40\ 25 S.$	$40\ 26-27'$	$40\ 21 \mid 40^\circ\ 30' S.$
$m\ k$	$53\ 23\frac{1}{2}$
$m'\ d$	$99\ 56$	$99\ 58\ \text{to}$ $100\ 2\ 99^\circ\ 50'$
$m\ c$	$81\ 39\frac{1}{2}$	$81\ 41$	$81\ 38 S.$	$81^\circ\ \text{to}\ 52^\circ$

A distinct cleavage runs parallel to the faces of the form 001. It was not possible to determine the direction in the crystal of the acute bisector of the optic axes.

III. *Terpene Hydrate* (fig. 6).

Crystals of terpene hydrate, made by Dr. Tilden from dextro- and from lævo-rotatory turpentine-oil, exhibit no distinction in the character of their forms. They are in fact crystallographically identical, belonging to the orthosymmetrical (or rhombic) system, and presenting the forms $a\ \{100\}$, $m\ \{110\}$, $o\ \{111\}$, and $k\ \{101\}$, and occasionally $b\ \{010\}$.

The crystals of terpene hydrate have been measured by List (*Pogg. Ann.* vol. lxvii. p. 364), by Rammelsberg (*Crystallogr. Chem.* p. 406, and Suppl. p. 227), and by Arzruni, who has also determined the optical characters of the substance. The measurements made by me accord very nearly with those obtained by Arzruni.

The parameters of the crystal are

$$a : b : c = 0.8082 : 1 : 0.4788,$$

the angles calculated from which elements, and the averages of those obtained by measurement on four crystals (two from each source), form the two columns in the following Table:—

Angles.	By calculation.	Arzruni.	By measurement.
am	$= 51^{\circ} 3' 30''$	$51^{\circ} 3' 30''$	$51^{\circ} 3\frac{1}{2}'$
mm'	$= 77 53$	$77 49 20$	$77 53$
mb	$= 38 56 30$		$38 56\frac{1}{4}$
ak	$= 64 26 30$		$64 26\frac{1}{4}$
kk	$= 51 8$		$51 18$
mo	$= 52 44$	$52 49 40$	$52 36 15$
ao	$= 67 37 30$		$67 37\frac{1}{4}$
oo'	$= 44 45$	$44 38 30$	$44 49$
bo	$= 61 54$		
ok	$= 28 6$	$28 1 30$	28
oo'	$= 56 12$	$56 8 30$	$56 2 30$

The faces m give in general excellent images; the faces o give a banded image. Arzruni's parameters are $0.80722 : 1 : 0.47640$.

XX. *On an Artificial Diopside Rock formed in a Bessemer Converter by Mr. Percy Gilchrist. By N. S. MASKELYNE, F.R.S.**

MR. MASKELYNE drew the notice of the Society to the production of diopside on a considerable scale at Blænavon by Mr. Percy Gilchrist and Mr. Sidney Thomas, during some experiments those gentlemen conducted having in view the elimination of phosphorus in the Bessemer converter. The artificial diopside was produced in a downdraught kiln at a very intense and prolonged heat—the kiln being lined with silica bricks, which were in contact with a moderately aluminous and siliceous magnesian limestone. The product resulting from the action of the bricks on the limestone occurs in large masses, portions of which present the appearance of an interlaced mass of glistening crystals of a grey hue.

Here and there, in hollows, minute crystals are met with

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presenting faces; and on placing one of these on the goniometer the nature of the mineral was placed beyond doubt.

It is, in short, diopside, with the forms m , $\{1\ 1\ 0\}$; b , $\{0\ 1\ 0\}$; o , $\{2\ 2\ 1\}$; s , $\{1\ 1\ 1\}$, as is seen from the following comparison of the calculated with the measured angles:—

	Calculation.	Found.
$m\ m' =$	$87^{\circ}\ 5'$	$87^{\circ}\ 18'\ 45''$
$m\ b =$	$43\ 32\frac{1}{2}$	$43\ 36$
$a\ b =$	$46\ 27\frac{1}{2}$	$46\ 21$
$m'\ o =$	$35\ 25$	$35\ 39\frac{1}{2}$
$m'\ s =$	$58\ 46$	$59\ 38$
$m\ s =$	$78\ 56$	$78\ 44$

Two specimens of this artificial rock were analyzed by Mr. Gilchrist, and gave the numbers in columns 1 and 2:—

	(1)	(2)	(3)	(4)
FeO . . .	1·63	1·63	1·38	
Al ₂ O ₃ . . .	2·47	2·47		
CaO . . .	19·50	21·00	25·05	25·93
MgO . . .	14·45	16·49	17·36	18·52
SiO ₂ . . .	63·00	58·75	56·03	55·55
	<u>101·05</u>	<u>100·34</u>	<u>99·82</u>	<u>100·00</u>

These analyses correspond very nearly to that of a diopside containing one equivalent each of calcium and magnesium; but with an admixture of silica in the one case of about 17, and in the latter case of 14·5 per cent. in excess.

This ingredient is doubtless a mechanical adjunct to the diopside, and is derived from the silica brick, to the presence of which the formation of the diopside is due. The portions of the mass in which the alkaline earths are in excess do not contain the diopside, and they gradually become slaked on exposure to the air. The composition of such an ideal diopside would be that indicated by the numbers in column (4), its formula being $(Ca\ \frac{1}{2}\ Mg\ \frac{1}{2})\ SiO_3$.

Column (3) represents the results of an analysis by Rammelsberg of a diopside from Retzbanya, which is given for comparison with that of the artificial diopside rock. The artificial production of an augitic mineral is no new fact; but the formation on a considerable scale of a veritable diopside rock appears to be as novel as it is interesting.

XXI. *Enstatite Rock from South Africa.*By N. S. MASKELYNE, *F.R.S.**

MR. MASKELYNE exhibited sections of a rock from two different localities in the Transvaal, which, when examined under the microscope, presented all the characters of a very crystalline enstatite without affording evidence of the admixture of other minerals; and this anticipation of its nature has been subsequently confirmed by Dr. Prevost in Mr. Maskelyne's laboratory at Oxford. The specimens from which the sections were made were collected by Mr. Dunn, who described the two rocks in question as forming hills of boss-like form at Korn Kopje, and at a place twelve miles south of Holfontein in the Witfontein Mountains, to the south of Lydenburg in the Transvaal.

The occurrence of a pure and massive enstatite rock is new to petrology, though rocks (such as lherzolite) are known in which enstatite is a very prominent ingredient mineral. Its occurrence in South Africa has, moreover, a special interest, since Mr. Maskelyne first asserted the enstatitic or bronzitic origin of the rock in which the diamonds occur in that region of the world. The serpentized mass of which the diamond-mines are composed was first shown, on crystallographic and microscopic grounds, to have contained, and in no inconsiderable degree to have consisted of, bronzite (ferro-magnesian enstatite); and this was confirmed by actual analysis, by Dr. Flight, of the grains of bronzite still left unaltered in that rock. (See *Quart. Journ. Geol. Soc.* vol. xxx. p. 406, 1874.) The diamantiferous rock, however, contains other minerals, and must have been not very dissimilar to lherzolite. The enstatite rock from the neighbourhood of Lydenburg is, on the other hand, composed nearly exclusively of that mineral, which is chiefly familiar to the mineralogist from its being an important ingredient of meteorites, and is likely, in other respects, to become recognized as a more frequent ingredient of rocks than has hitherto been anticipated, though, like the kindred mineral olivine, the more ferruginous kinds have frequently undergone a more or less complete serpentization. The Baste rock in the Hartz and the so-called pseudophite of Zdjär are known to be still rich in enstatitic mineral, though otherwise almost completely metamorphosed; and in many serpentine, such as that of the Lizard, crystals whole or in part still survive as witnesses to the original nature of the rock. Mr. T. Davies has also pointed out to the author of this notice that the eulysite from Tunaberg presents, under the microscope, the characteristic features of enstatite or bronzite in one of its

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ingredient minerals—the other minerals associated with it being olivine and garnet, as is the case in some specimens of lherzolite, though the two rocks are quite dissimilar in aspect.

The following Table exhibits the analysis (1) of the Korn-Kopje rock by Dr. Prevost, in which all the iron is assumed to be in the ferrous condition; (2) of a rock from the Radauthal (occurring in bastite) in the Hartz, by Streng (*Jahrb. Min.* 1862); (3) of that in the lherzolite of L. Lherz, by Damour (*Bul. Géol.* xxix. p. 413):—

	(1)	(2)	(3)
Silica	53	54.15	54.76
Alumina	2.6	3.04	4.90
Ferrous oxide	9.27	12.17	9.35
Manganous oxide . . .	2		
Magnesia	25.5	28.37	30.22
Lime	6.6	2.37	
	98.97	100.10	99.23

The excess of lime in the analysis is probably traceable to an augitic mineral, a diopside, probably present as an ingredient, although not yet recognized by the microscope in the sections made from the South-African rocks. In fact the microscope, so invaluable as an instrument for pioneering in the realm of petrology, is frequently an untrustworthy guide when too much relied on—that is to say, when the results obtained by it are not checked and confirmed, and in fact supplemented by the more tedious methods of investigation pursued in the laboratory.

XXII. On a *Horizontal Goniometer*. By VICTOR VON LANG*.

[Plate VII. figs. 7 & 8.]

THE instrument represented in fig. 8 owes its existence to the necessity of being able to measure refractive indices at different angles of incidence. This is of great importance when one wants, like Professor Stokes, to verify the theoretical formulæ of double refraction by experiment, or when one tries, on the contrary, to determine with the aid of these formulæ the constants of double refraction from observations on a prism of arbitrary position.

The measurements I made in order to determine the figure of the wave-surface in quartz near its axis belong to the first kind of researches; whereas the determination of the refractive indices of gypsum I have just finished gives an example of the second kind of researches, as for that determination one single prism was made use of.

* Communicated by the Crystallological Society.

The chief requisites of such an instrument are two concentric axes—the inner one carrying the vernier and the prism-table, the outer movement carrying the telescope and the graduated circle. A collimator with slit is, of course, fixed to the tripod of the instrument.

The two axes are not put into one another, as is done in geodetical instruments, for the sake of repeating the angles; but here the tripod (G) is terminated in a strong cone that forms the axis on which the circle (C) turns, whereas a hole in the centre of the cone supports the axis (A) of the vernier (*n*). Fig. 7 shows this arrangement in a sectional drawing.

Both axes may be clamped by the screws L L', and micro-metrically moved by the screws M M'. Suppose we clamp the inner axis with the prism, of which the angles of deviation (D) and of incidence (*i*) are to be measured. Let S be the reading of the vernier by direct observation of the slit, R the reading by observation of the slit after reflection from the first plane of the prism, and T the reading by observation of the spectrum. Then we have

$$D = T - S, \quad i = 90^\circ - \frac{1}{2}(S - R);$$

and the index of refraction in the direction given by the angle *r* is found from the known formulæ

$$r = \frac{\sin i}{\sin r}, \quad \tan\left(r - \frac{A}{2}\right) = \tan \frac{A}{2} \tan\left(i - \frac{A + D}{2}\right) \cot \frac{A + D}{2},$$

A being the angle of the prism.

As long as the relative position of the prism and the vernier is not altered, we have

$$S + R = C = \text{constant.}$$

For if we turn the prism (with the vernier of course) through an angle $+\chi$, all readings will increase in the same ratio; but the reflected image of the slit turns at the same time through an angle -2χ , so that the sum $S + R$ will indeed remain constant. We find by the quantity C,

$$i = 90^\circ + \frac{C}{2} - S;$$

and this formula allows us to find the angle *i* from the reading S. In this way it is possible to determine the angle of incidence even when it is very small; in which case it cannot be observed directly, as telescope and collimator cannot be brought so near to each other.

If we put the telescope to the slit and then turn the inner axis till the vernier gives $90^\circ + \frac{C}{2}$, then the light will fall per-

pendicularly on the first plane of the prism, the angle i being then evidently zero.

Of course the angle of the prism can also be determined with this instrument; and it is not necessary to alter any thing in its arrangement or to move the prism. We have only to clamp the telescope in a position of about 90° to the collimator, and then to observe the reflected image alternately on both faces of the prism.

In order to be able to use the instrument for other purposes, both telescope and collimator can be shifted parallel to their axes and be fixed by the screws $O O'$. If the instrument is to be used as a spectroscope, one may put conveniently before the objectives of telescope and collimator prisms with direct vision.

XXIII. *The Moist-Combustion Process: some Reactions of Alkaline Permanganate of Potash.* By J. ALFRED WANKLYN and W. J. COOPER*.

FOLLOWING up our investigations (the results of which were communicated in the June Number of this Journal, and in the Chemical Section of the British Association at the Dublin Meeting last year), we have now to announce that we have overcome one of the difficulties which stood in the way of giving to our process absolute generality of application to all organic substances.

It will be remembered that, starting with the organic substance in aqueous solution, we showed that permanganate of potash and excess of alkali burnt down the organic substance to the state of carbonates, oxalates, and water; and we proposed to render the solution acid, and so, as was well known, would burn down the oxalates to carbonates. At the Dublin Meeting we announced that although this answers very well in many cases, yet in certain classes of cases acetates appeared among the products of oxidation; and when once produced, acetates resisted further oxidation.

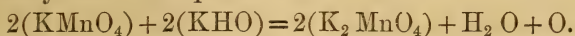
We have now managed to oxidize the acetates, by the simple process of using considerable excess of permanganate and raising the temperature some 60 or 80 degrees above the boiling-point of water. Under these conditions acetate of potash yields carbonate of potash, and apparently no oxalate whatever.

There is a difficulty, however, attendant on the use of these

* Communicated by the Authors.

high temperatures; and on this occasion we wish to explain this difficulty and how it has been overcome.

The difficulty arises from the fact that, at temperatures very little above 100°C ., a mixture of pure permanganate of potash and caustic potash evolves oxygen gas. This fact we have very carefully ascertained, both by noting the diminution in oxidizing-power which the solution shows after being heated to 180° in the oil-bath, and by actually collecting and measuring the oxygen gas which was evolved during the heating in the oil-bath. The gas is evolved very freely at temperatures even below 140°C .; and the numerical results accord very fairly with this equation:—



Here, as will be observed, the permanganate of potash is represented as losing one fifth of its *active* oxygen, and yielding manganate of potash.

It has long been known that at very elevated temperatures, at temperatures bordering on low redness, permanganate of potash parts with oxygen and forms manganate of potash—that, in point of fact, at these elevated temperatures manganate, and not permanganate, is the stable form of combination; but we believe this easy evolution of oxygen at temperatures a little above the boiling-point of water is quite a novelty. For the moist-combustion process it would be a very uncomfortable novelty if we were unable to stop the evolution of the gas by a convenient device, since the alkaline permanganate would cease to keep a trustworthy record of the consumption of oxygen during the process.

We have, however, to add that we can stop the evolution in a most convenient manner. We mix some hydrated bin-oxide of manganese with the permanganate and alkali; and then there is no evolution of oxygen. Why this addition should be effective is obvious; and the chemist will have no difficulty in understanding that the necessity of having to make such a condition does not damage our process.

We have to record, as an interesting reaction, the behaviour of green oxide of chromium with strongly alkaline solution of permanganate of potash. It acts very readily, and yields chromate of potash and hydrated bin-oxide of manganese; this takes place at temperatures even below the boiling-point of water. We are following up this experiment, and hope to make new and rare metallic acids.

XXIV. *Notices respecting New Books.*

The Speaking Telephone, Talking Phonograph, and other Novelties.

By GEORGE B. PRESCOTT. *Fully Illustrated.* New York: D. Appleton and Company. 1878. (8vo, pp. 431.)

THIS volume is divided into thirteen chapters, of which the first nine contain an account of the Speaking Telephone, the tenth is devoted to the Talking Phonograph, and the last three to the "other novelties," viz. Quadruple Telegraph, Electric Call Bells, and the Electric Light. The subjects comprised in this list are at present matters of general interest; and a book which gives a good deal of information respecting them will doubtless obtain a large number of readers. The printing and general appearance of the book are much in its favour. The illustrations, which are numerous, are well executed, and will help the reader to form a distinct conception of the objects they represent.

It will be observed that the largest part of the volume is devoted to the telephone. The reason of this we may state in the author's words:—"The question as to whom we are indebted for the telephone is one which, in consequence of the conflicting statements that have appeared from time to time, is, to say the least, extremely puzzling. We have, therefore, endeavoured to give it the attention its importance demands, in order to arrive at a true solution of the problem, and, in doing so, have taken every opportunity to consult all available authorities on the subject. No effort has been spared in our investigation to obtain all the facts as they are; and these are now given as we have found them, without favour or prejudice" (p. i). Mr. Prescott's grammar is not quite perfect; but his meaning is sufficiently plain; and, it comes to this, that he has written his book for the purpose of advocating the claims of Mr. Gray to be considered the inventor of the Telephone. This appears very clearly in the first chapter, which gives a general description of the instrument in its principal forms. After an account of Reiss's Musical Telephone, which transmits the pitch but not the variations in the intensity and other qualities of the tone, the author describes the principles on which three Telephones are constructed. All three agree in the main point that vibrations caused by the voice of the sender are made to transmit electric waves of varying intensity through a circuit, and these reproduce the vibrations at another point and thereby transmit the sounds to the receiver with all their variations of pitch, intensity, and quality. In Gray's telephone this is effected by causing the current to pass through a fluid from one wire to another wire, which moves with the membrane put into vibration by the voice, and thus the fluid space traversed varies with the amplitudes of the vibrations and subjects the current to a varying resistance. In Bell's telephone the membrane carries a light permanent magnet placed near the poles of an electromagnet; the vibrations of the membrane therefore induce a succession of magneto-electric pulsations varying in intensity with their amplitudes,

and causing corresponding variations in the intensity of an electro-magnet placed at another point of the circuit, where these variations produce vibrations precisely resembling the original vibrations. In Dolbear's telephone the electromagnets are replaced by permanent magnets surrounded by helices; the receiving and transmitting diaphragms are precisely alike, and, in place of a membrane, consist of small disks of thin iron plate. The vibrations of the one plate induce magneto-electric currents, which vary the force of the magnet at the other end of the circuit and thereby produce corresponding vibrations in the other plate. The peculiarity of this form of the instrument is that its action is reciprocal; either end may be the receiver, and the other the transmitter. The chapter also contains an account of several of the improved forms of the Telephone; but with these we are not concerned. The main point consists in the dates assigned to these three inventions; and these dates, we may observe, are by no means stated with due distinctness. Our author tells us that Mr. Elisha Gray, of Chicago, invented his method subsequently to the spring of 1874 (pp. 14, 15); that Professor A. G. Bell, of the Boston University, exhibited his instrument, in the summer of 1876, at the Centennial Exhibition in Philadelphia (p. 16); and that Professor A. E. Dolbear, of Tufts College, made his improvements in the ensuing autumn (p. 19). This is, perhaps, a sufficient statement of Mr. Prescott's case. In the following chapters he allows the several claimants to speak for themselves:—Chapter ii. contains Professor Bell's account of his researches, in a lecture delivered to the Society of Telegraphic Engineers; chapter v. Mr. Gray's account of his experimental researches; and chapter viii. an abstract of "Researches in Telephony," by Professor A. E. Dolbear.

The above statement suggests one or two remarks:—*First*. It does not seem clear from chapter viii. that Mr. Dolbear claims priority of invention; but if he does, the claim does not appear to rest on documentary evidence. *Secondly*. In chapter v. we fail to find any confirmation of the date apparently given on page 14 for Mr. Gray's invention, which we have very briefly described above, viz. the spring of 1874*. We do, indeed, find a very surprising coincidence, viz. "a verbatim copy" of Gray's specification and of Bell's specification, both filed in the United-States Patent Office on the same day, viz. Feb. 14, 1876. In the former of these documents Mr. Gray says, "I claim as my invention the art of transmitting vocal sounds or conversations telegraphically through an electric circuit" (p. 205); while in the latter Mr. Bell claims, *inter alia*, "the method of, and apparatus for, transmitting vocal or other

* Strictly speaking, Mr. Prescott says that in the spring of 1874 Mr. Gray "invented a method of electrical transmission by means of which the intensity of tones, as well as their pitch, was properly reproduced at the receiving station" (p. 14), and that subsequently he invented the Telephone above described. "Subsequently" may, of course, mean any thing. We think, however, that the suggestion of the passage (pp. 14, 15) is as above stated. In chap. v. there is no trace of the Telephone until a few weeks before the date of the specification, viz. Feb. 14, 1876.

sounds telegraphically as herein described" (p. 215). *Thirdly.* It must be borne in mind that Mr. Prescott distinctly advocates Mr. Gray's claim. This, indeed, is clear from what has been already said; and if further confirmation were needed, it would be found, for example, in the notes on p. 71 and p. 73, and in the passage at the end of chapter v. on p. 217: he by no means keeps to the attitude of a candid inquirer, which he appears to assume in his preface. On this side of the Atlantic we shall be very much surprised if Mr. Bell's claims to priority of invention are successfully contested; still the matter is one which must be adjudicated in the United States.

Of the remaining parts of the volume there is not much to say. They seem to us by no means well drawn up. From time to time the composition is very slovenly. In the contents there are both excess and defect. For instance, in the chapter on the Electric Light a good deal is said which the reader might be presumed to know, while the parts relating to "novelties" are cut very short. In several cases elaborate figures are given marked with half the letters of the alphabet, and plainly designed to accompany detailed explanations; the explanations, however, are not there (figs. 202, 203, and 207 are instances). Another very serious fault—and the more serious as the book deals in part with evidence—is the way in which documents are quoted and extracts made from other writers. It is never easy, and sometimes impossible to tell whether an extract is given verbatim, and even where it begins and where it ends. Still, with all its faults, the book is one of considerable interest, and will doubtless find many readers.

XXV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 67.]

Jan. 8, 1879.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On some Tin-deposits of the Malayan Peninsula." By Patrick Doyle, Esq., C.E. (Communicated by the Rev. T. Wiltshire, M.A., F.L.S., F.G.S.)

The tin-ore of the Malayan peninsula is obtained from "stream-works" in an alluvial plain extending between a range of granitic mountains and the sea. The author describes the mines of the district of Larut Perak. The ore is got in open workings at an average depth of about 10 feet. The tin-bearing stratum has an average thickness of 4·87 feet; it is overlain by stratified sand and clay, and rests upon either porcelain-clay or, sometimes, a sandstone. The ore varies from a fine sand, near the sea, to a coarse gravel, near the mountains, and is mixed with quartz, felspar, mica, and schorl. The author is of opinion that the stratum of ore has been derived from the granite of the mountain-range (in which it still occurs in veins) by denudation, and under conditions which still exist, though in a modified form.

2. "Description of Fragmentary Indications of a huge kind of Theriodont Reptile (*Titanosuchus ferox*, Owen), from Beaufort West, Gough Tract, Cape of Good Hope." By Prof. R. Owen, C.B., F.R.S., F.G.S.

3. "Notes on the Consolidated Beach at Pernambuco." By J. C. Hawkshaw, Esq., M.A., F.G.S.

The consolidated beach at Pernambuco, which has already attracted considerable notice, is a ridge of sandstone from 25 to 75 yards wide, and, as shown by borings made under the author's direction, from 10 to 13 feet thick. The landward or higher edge is nearly at the spring-tide high-water level; and it slopes seaward, the river (with a depth of 28 feet at low water 60 feet from the rock) flowing along the former face. The rise and fall of spring tides is 7 feet. Beneath the above rock is a stratum of sand with shells and stones about 8 feet thick, and then a second layer of sandstone rock.

The consolidated beach is cemented by carbonate of lime, which the author considers to have been deposited by the action of water percolating through the rock, probably when the level of the land differed somewhat from what it is at present. He thinks it possible that this and other similar beaches on the Brazilian coast may mark periods of repose in the slow vertical movements which the coast has undergone.

XXVI. *Intelligence and Miscellaneous Articles.*

NOTE ON ELECTROMAGNETS IN TELEGRAPHY.

BY OLIVER HEAVISIDE.

IN a recent Number of this Journal I stated:—"On telephonic circuits the reduction in current-strength is nearly inversely proportional to the pitch of the sound," &c. This is only true for a constant electromotive force. Thus it would be correct for a battery telephone. The strength of very rapid currents from a battery through an electromagnet is nearly inversely proportional to the rapidity. But in an electromagnetic telephone, although the electromotive *impulse* produced by a semivibration of the iron disk of the sending telephone is *constant* for all pitches, provided the amplitude of the vibration is the same, yet when the semivibration is executed in half the time, the mean value of the electromotive force is doubled. Thus, instead of the second partial tone of a continuous sound being weakened nearly twice as much as the first, it will not be quite so much weakened. Γ being the current, R the resistance, L the electromagnetic capacity, and $m = \frac{2\pi}{T}$, where T is the time of a complete vibration, then

$$\Gamma = \frac{mE}{\sqrt{R^2 + L^2 m^2}}$$

in the case of a Bell telephone, where E is proportional to the amplitude of vibration of the sending disk. Keeping E constant, Γ increases slightly with m . Also, when R is increased, Γ is reduced more for low pitches than for high.

ON TWO NEW FLUORESCENT SUBSTANCES. BY E. LOMMEL.

From Dr. Th. Schuchardt, of Görlitz, I received, some time since, two new fluorescent substances, the examination of which (by the method I have previously described) gave the following results.

1. *Anthracene-Blue*.—The deep-blue etheric solution fluoresces intensely olive-green. The absorption-spectrum shows four dark bands, the darkest parts of which are situated at 36, 55, 69, and 86, respectively, of Bunsen's scale, and a darkening of the violet end, commencing a little before H, while the blue and the greater part of the violet appear almost unaffected. Of the absorption-bands the second and the third (55 and 69) are by far the most intense and about equal in intensity; then follows, in the order of intensity, the rather feeble first (36); and last comes the still feebler fourth (86). The spectrum of the light of fluorescence begins faint at 27, distinct at 30, and extends distinct as far as 70, faint as far as 74. It shows three maxima, at 40, 52, and 63, separated by two very clearly marked minima (at 45 and 57), and hence appears to consist of three bright bands—one red, one orange-yellow, and one green-yellow—of which the middle one a little exceeds the others in brightness. The fluorescence-spectrum projected upon the liquid commences faintly at 47, and shows two very bright fluorescence-bands corresponding to the two most intense absorption-bands, divided by a tolerably dark interspace, and strongly distinguished from one another by their different colouring; namely, the first is entirely orange-coloured, the second yellow-green. A third, but much fainter, brownish olive-green-coloured maximum at 86 corresponds to the fourth absorption-band; the first, on the contrary, has no part in the fluorescence. In the blue and the violet, from F to shortly before H, the fluorescence is very slight, olive-green to reddish, and scarcely perceptible; only just before H does it again become more intense, and extends with an olive-greenish tone into the ultra-violet. We have here, therefore, a fluorescing substance, with which *the blue and the greater part of the violet rays exert only an extraordinarily feeble, while the orange-yellow and yellow-green rays exert a very intense excitant action*. If we excite with homogeneous light, descending gradually to continually less refrangibility, we observe that the entire fluorescence-spectrum is excited by all the rays down to 58—by the ray 58, for example, not merely the portion from 30 to 58, but also the more refrangible from 58 to 70. The rays below 58, however, do not excite the yellow-green part of the light of fluorescence (58 to 70), but only the red and orange-coloured part, but this also entirely: for example, if we excite with the ray 48, the fluorescence-spectrum from 30 to 55 is distinctly seen; light which has passed through three red glasses, and only reaches to 50, excites still very distinctly up to 57. Accordingly the fluorescence-spectrum of anthracene-blue consists of two parts, separated by the minimum at 57, neither of which obeys Stokes's rule, and the second, more refrangible, is excited only by the rays above 57. Anthracene-blue therefore behaves like a mixture of

two fluorescent substances of the first class*, one of them characterized by the absorption-band 55 and orange-yellow fluorescence (30 to 57), the other by the absorption-band 69 and yellow-green fluorescence (57 to 70)†.

2. *Bisulphobichloranthracenous Acid*.—The faintly brownish-yellow-coloured etheric solution presents no absorption-bands, but only a slight absorption of the blue and a stronger of the violet rays. It fluoresces at its surfaces a very beautiful blue, from its interior greenish. The spectrum of its fluorescence-light extends from 30 to 162, (therefore to the violet end), and shows four maxima—viz. between 70 and 80, at 109, 131, and 150, of which the first three appear about equally intense, the last somewhat less so. Between these maxima are feebly marked minima to be perceived at about 98, 117, and 140. The fluorescent spectrum commences (faintly) at about F, and extends, with an olive-greenish tone of colour and slight intensity of light, up to about 150, here almost suddenly becomes a beautiful blue and very bright, attains its greatest brightness, immediately before H, and reaches, with the same blue colour, far into the ultra-violet. The fluorescence is of the second kind, *i. e.* follows Stokes's rule.—Wiedemann's *Annalen*, 1879, No. 1, pp. 115–118.

ON THERMAL RADIATION AT HIGH TEMPERATURES.

BY J. L. SORET.

In some researches the results of which I have previously made public‡, I arrived at the conclusion that Dulong and Petit's law of thermal radiation ceases to be found true at very high temperatures. Since then I have made, according to other methods, a few similar experiments, which I have not yet published, because I wished to complete and render them more exact. Long ago I spoke of them to M. Raoul Pictet, who, now engaged in a critical investigation on solar heat, has asked me for some information on the subject; and this has induced me to give the following compendious summary of those experiments, imperfect as they are.

When an electric current is caused to pass through a conducting wire, the temperature of the wire rises until the heat which it loses by radiation, by contact with the air, and by conductivity at the points of attachment becomes equal to the heat evolved in its interior by the electricity. We can, on the one hand, estimate this last quantity of heat, and, on the other, by estimating the temperature taken by the wire, calculate the quantity of heat which it ought to emit according to the law of Dulong and Petit.

* Compare Wied. *Ann.* iii. p. 125 (1878).

† In accordance with the theory (see Wied. *Ann.* iii. p. 251, 1878), each of these two parts of the fluorescence-spectrum would represent the emission-spectrum belonging to the corresponding absorption-bands displaced somewhat downwards, or each of the two absorption-bands would have to be regarded as the "reversal" of the fluorescence-band belonging to it.

‡ *Archives des Sciences Phys. et Naturelles*, 1872, t. xlv. p. 220, t. xlv. p. 252; 1875, t. lii. p. 89 &c.; 1876, t. lv. p. 217. *Phil. Mag.* [IV.] vol. 1. p. 155 (1875).

Seeking to make some comparisons of this sort, I have employed the dynamoelectrical machine of the Geneva University, which was set to work by first passing the current through a Serrin lamp, or any other equivalent resistance, until the machine had acquired its normal velocity. Then, by means of a commutator, the current was directed through a platinum wire, while the lamp was excluded from the circuit. The wire grew rapidly hot, and soon melted.

The dynamoelectric machine is driven by an hydraulic motor of nominal four-horse power. All this force is far from being employed; but let us suppose that it is. A four-horse power corresponds to a work of 18000 kilogrammetres per minute, equivalent to 42·3 calories. Suppose, further, that the dynamoelectric machine is perfect, and that all the motive work is converted into electric current; neglect the heat evolved in the machine itself and in the conductors that convey the current to the platinum wire, upon which we will suppose that the whole effect is concentrated. That wire will therefore receive, as a maximum, 42·3 calories (a number which in reality is certainly four times too much).

The diameter of the platinum wire was 0·31 millim.; therefore its cylindrical surface was 1 square millim. per millim. of current. The length of the wire, in three experiments, was 385 millims. It was melted in a few seconds, and broke at several points. On afterwards examining its fragments, traces of liquefaction were everywhere recognized. We may therefore conclude that the whole of it was raised to the temperature of fusion of platinum, which, according to the lowest estimates, exceeds 1700°. The total surface of the radiation was 385 square millims.; but let us reckon only 3 square centims., in order to make a liberal allowance for the circumstance that the two extremities may be cooled a little by contact with the electrodes; and, finally, let us neglect the loss of heat by contact with the air. Upon these data let us calculate the quantity of heat emitted by this wire according to the law of Dulong and Petit, taking the formula given by Pouillet,

$$e = g f \alpha^t, —$$

where e is the quantity of heat, one gram of water heated 1° being taken as unit; g a constant, of which the value is 1·146 when the square centimetre is taken for the unit of surface, and the minute for the unit of time; f is the emissive power; α is the constant of Dulong and Petit, or 1·0077; t the temperature. We will adopt the number given for the emissive power by MM. La Provostaye and Desains, $f=0·092$.

On making the calculation, putting $t=1700$, we find 48541 units of heat, or 48·541 calories, per square centimetre of radiating surface. For the whole of the wire, then, 145·623 calories should have been evolved if the law of Dulong and Petit were applicable, while the motor could supply only 42·3 at the most. The difference is enormous.

Another method, capable of giving much more exact if not more striking results, consists in taking a pile, a tangent-compass, and completing the circuit by a platinum wire of a certain diameter and a

certain length, which becomes heated to a temperature which can be estimated in different ways. The deflection of the compass is observed when it has become stationary. The wire is then replaced by a thinner one, of which the length is varied until the compass gives the same deflection as before; the rest of the circuit remains unchanged. The resistance of the stout and long and that of the short and thin wire are evidently equal, and the quantities of heat evolved in their interiors are the same.

Operating thus with two platinum wires taken from the same sample, one of them 0.62 millim. in diameter and 538.7 millims. in length, and the other of 0.31 millim. diameter and 77.7 millims. length, the same deflection was obtained. The ratio of the two radiation-surfaces is therefore

$$\frac{2 \times 538.7}{77.7} = 13.9.$$

The radiation per unit of surface of the thin wire was therefore 14 times as much as that of the thick wire (neglecting the heat taken away by contact of the air).

On the other hand, the thick wire was raised to a temperature much below red heat (it did not char wood), while the thin wire became white-hot. If we admit that the difference of temperature was only 600° (a figure certainly too low), the heat emitted per unit of surface by the two wires should have been, according to the law of Dulong and Petit, in the ratio of 1 to 100, and not 1 to 14 as was given by the experiment*.

I made some more trials by a very different method. A globule of platinum, as large as possible, placed in a magnesia cupel, is fused by the flame of a little blowpipe with illuminating-gas and oxygen well mixed. The heat evolved by the gas in burning is about 5600 calories per cubic metre; therefore, if the pipe consumes 1 litre per minute, neglecting the losses (which are necessarily considerable), the maximum of heat that can be communicated to the platinum is 5.6 calories. Then, if we estimate the radiating free surface of the metallic globule, which is raised to the temperature of fusion of platinum at least, we can calculate the quantity of

* M. Ed. Becquerel also arrived at similar results, although to my knowledge he has not discussed them from the point of view occupied by us. He operated upon a platinum wire placed *in vacuo* and passed through by an electric current of varying intensity; and he calculated the heat evolved internally by multiplying the square of the intensity of the current by the resistance of the wire, measured at each experiment. Passing thus from a temperature much below red heat to one near the melting-point of platinum, those quantities of heat did not change in the ratio of 1 to 13.—*La Lumière*, t. i. p. 92.

We may also cite Professor Tyndall's experiments, who, on measuring with a thermoelectric pile the intensity of a single kind of obscure radiations, found that the energy of the radiation of a platinum wire changes in the ratio of 6 to 122 in passing from a temperature below red heat to that of an intensely white heat. According to the law of Dulong and Petit, the ratio ought to have been as 6 to 600, supposing that the difference between these two temperatures amounted to only 600°.—*La Chaleur*, trad. franç. p. 414.

heat that ought to have been emitted according to the law of Dulong and Petit. It is found to be much more than 5.6 calories.—*Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles*, Jan. 15, 1879, t. i. pp. 86-90.

ON ELECTROCHEMICAL ACTIONS UNDER PRESSURE.

BY A. BOUVET.

In a series of about fifty experiments, each lasting several hours, and during which I have been able to produce with extreme facility pressures of 100, 200, 300 atmospheres, &c., I constantly recognized the existence of the two following laws:—

(1) The decomposition of water by a current is independent of the pressure;

(2) The quantity of electricity necessary for decomposing one and the same weight of water is sensibly the same, whatever may be the pressure at which the decomposition is effected.

I have ascertained by experiment that the mechanical theory of heat perfectly accounts for these two laws. Thus, by taking advantage of the fact that gases produced in the midst of water are obtained at a sensibly fixed temperature, I verified the formula which represents the work expended for the compression of gases without change of temperature:—

$$T = PV \int \frac{dv}{v} = PV \log \text{hyp} \frac{V_1}{V}.$$

T, work = V_1 , final volume after the expansion;

V, volume of the compressed gas;

P, pressure.

The result of the experiment made with gases at 200 atmospheres agreed perfectly with the result calculated after the above formula.

For the sake of brevity, I will only add the following:—

1st. The oxygen and hydrogen, at whatever pressure, are liberated with equal facility.

2nd. These two gases may be produced in a single test-tube or in two; in neither case are there any secondary phenomena determining even *partial* recombination, as has hitherto been believed. The *precise* and *constant* indications of the manometer, the regular increments of pressure, ascertained from minute to minute *during several consecutive hours*, leave no doubt on this point.

3rd. The oxygen and hydrogen, when they are collected in one and the same test-tube, even under considerable pressure, and although constituting the explosive mixture, are not at all dangerous.

The electrodes I employed were of platinum. I always took care to let them be completely immersed.

In the course of my experiments, lasting several months, I never ascertained any appreciable variation of temperature, although I sometimes employed currents possessing very energetic tension.—*Comptes Rendus de l'Académie des Sciences*, Dec. 30, 1878, tome lxxvii. p. 1068-69.

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XXVII. *Acoustical Observations.* II.
By LORD RAYLEIGH, *F.R.S.**

Pure Tones from Sounding Flames.

THE best approximation to a pure tone is doubtless that given by a fork held over a suitably tuned air resonator; but unless the vibrations are maintained, the sound is of but short duration, and varies in intensity throughout. On the other hand the introduction of an electro-magnetic maintenance (as in Helmholtz's vowel experiments) somewhat complicates the apparatus. For many purposes extreme purity and constancy of pitch are not important; and thus an arrangement which shall be simple and easy to manage, even though less perfect in its operation than a tuning fork, is still a desideratum.

During the last year I have often used with good effect air resonators whose vibrations were maintained in a well-known manner by hydrogen-flames. In the common form of the experiment an open cylindrical tube is employed as resonator, and gives a sound, usually of a highly compound character. In order to obtain a pure tone, it is only necessary to replace the tube by a resonator of different form, such as a rather wide-mouthed bottle or jar; but a difficulty then arises from the progressive deterioration of the limited quantity of air included. A better result is obtained from a tube with a central expansion, such as a bulbous paraffin-lamp chimney, which allows of a through draught, and yet departs sufficiently

* Communicated by the Author.

from the cylindrical form to give a pure tone. For ready speech, it is sometimes necessary to restrict the lower aperture, *e. g.* by a bored disk of wood attached with wax. Another plan which answers very well is to block the middle of a cylindrical tube by a loosely fitting plug. The tubes that I used are of cast iron, and were plugged by rectangular pieces of wood provided with springs of brass wire to keep them in position. The length of the plug may be about two diameters of the tube; the length of the tube itself should be about twelve diameters. In all cases the best result requires that the tubes through which the hydrogen is supplied be of suitable length, and be provided with suitable burners. These may be made of glass, and are easily adjusted by trial.

For ordinary purposes a common hydrogen-bottle is sufficient; but the note is rather more steady when the hydrogen is supplied from a gas-holder. In this way I have obtained pure tones, giving with tuning forks pretty steady beats of more than two seconds' period. When the intensities are nearly equal, the phase of approximate silence is very well marked.

Points of silence near a wall from which a pure tone is reflected.

On this subject there are two papers by N. Savart*, who advances views very difficult of acceptance. A criticism of some of Savart's positions was published soon after by Seebeck; but the question does not appear to have been thoroughly cleared up.

One source of confusion is imperfect recognition of the fact that the positions of the silences depend upon the nature of the apparatus used for the investigation. In the case of the ear a silence requires that there be no variation of pressure at the open end of the ear-passage, whether it be in its natural state, or prolonged by a tube fitted into the external ear. The addition of a small cone or resonator will not affect the truth of this statement. Thus, if the influence of the head and body of the observer acting as simple obstacles be put out of account (as may fairly be done when a tube is used), the silences occur at distances from the wall which are *odd* multiples of the quarter wave-length†. On the other hand, if a membrane simply stretched over a hoop and held parallel to the wall be used as the indicator, the positions of zero disturbance are at distances from the wall equal to *even* multiples of the quarter wave-length.

In the theory of organ-pipes the places of zero velocity

* *Ann. d. Chim.* lxxi. 1839, xl. 1845.

† The waves are here supposed complete. Savart's "ondes" are only half as long.

and of maximum pressure-variation are usually called *nodes*; and the places of zero pressure-variation and of maximum velocity are called *loops*. If we retain this nomenclature, we may say that silences as investigated by the ear occur at loops, and that the maximum sound is found at nodes; but in Savart's papers the silences are identified with *nodes*. Moreover the difference is not one of words merely; for Savart considers that (apart from the effects of obstacles) the silences are to be found at distances from the wall which are *even* multiples of the quarter wave-length. A large part of his work is thus an endeavour to bring the facts into accordance with a mistaken theoretical view.

When the median plane is parallel to the wall, the obstruction presented by the head displaces considerably the positions of the silences. In his first paper Savart proposes to add 27 mm. to the measured distances between the external ear nearer to the wall and the wall itself, in order to take account of the interval between the external ear and the sentient apparatus. In the case of the ear further from the wall a similar distance is to be subtracted. I am at a loss to understand how the situation of the sentient apparatus can be supposed to be an element in the question at all. Every thing must surely depend upon whether there is or is not a variation of pressure at the outer end of the ear-passage. In the second paper Savart takes (as it appears to me) a further step in the wrong direction. He states that the positions of the silences are the same, whether they be observed with the ear nearer to the wall, or with the ear further from it, and draws the conclusion that the part of the head with which we have to deal is that situated in the median plane midway between the ears. Having already added 27 mm. to his measurements (in the case of the ear nearer to the wall), to take account of the distance between the external ear and the labyrinth, he now adds 50 mm. more. By this artificial treatment the distances of the silences from the wall are made to agree with the series of even multiples of the quarter wave-length, though considerable anomalies remain unexplained.

There can be no doubt, I imagine, that Savart's theoretical views are quite erroneous, and that what has to be explained by the action of the head as an obstacle is the displacement of the silences from the loops, and not from the nodes. An exact theoretical investigation of this subject is of course out of the question; but some information bearing upon it may be obtained from a calculation given in my 'Theory of Sound,' § 328, relating to the character of the obstruction to sound presented by rigid spheres. It appears that if a source of sound

be situated at the surface of a sphere whose circumference is moderate in comparison with the wave-length, the *phase* (which is the element on which the phenomena under consideration principally depend) at a distance is approximately the same as if the source were moved outward from the surface through a distance equal to half the radius, and the sphere were removed altogether. By the theorem of reciprocity, § 294, it follows that in the case of reflection of plane waves there is a silence at the point on the surface of the sphere nearest the wall when, not this point itself, but another further from the centre by half the radius, is distant from the wall by an odd multiple of the quarter wave-length, provided that the distance between the sphere and wall be not too small a multiple of the radius. Instead therefore of adding with Savart 27 mm., or 77 mm., to the observed distances in the expectation of so arriving at even multiples of the quarter wave-length, we ought rather to subtract some such distance as 50 mm. in the expectation of arriving at odd multiples of the same quantity.

The following are some of Savart's results given in the first paper :—

Designations des divers points.	Distances des points à la paroi. mètres.
Paroi.	0·000
1 ^r ventre.	·148
1 ^r nœud.	·373
2 ^e ventre.	·716
2 ^e nœud.	1·000
3 ^e ventre.	1·358
3 ^e nœud.	1·615
4 ^e ventre.	1·997
4 ^e nœud.	2·275

If we subtract $27 + 50 (=77)$ from Savart's numbers for nodes we get

·296,	·923,	1·538,	2·198,
corresponding to			
$\frac{1}{2}(\cdot619),$	$\frac{3}{2}(\cdot619),$	$\frac{5}{2}(\cdot619),$	$\frac{7}{2}(\cdot619),$
or to			
·309,	·927,	1·546,	2·163 ;

·619 being the value of the half-wave (onde). The "ventre" between the wall and the first node does not belong to the regular system at all.

From a theoretical point of view, it appeared to me highly improbable that the silences for the two ears should occur in the same position of the head, except perhaps in the case of a

particular wave-length equal to about three diameters; and laboratory experiments with steadily maintained tones had made me familiar with the phenomenon of sounds apparently transferring themselves from one ear to the other when the head is moved; but I thought it desirable to try a few experiments in the open air especially directed to the examination of this point.

The source of sound was a lamp-chimney and hydrogen-flame, as described above, of pitch e' flat, so that the quarter wave-length was about eleven inches. The apparatus was placed at distances varying from about 18 to 50 feet in front of a tolerably flat wall; and the observer, with one ear stopped, investigated the positions of the silences, holding the middle plane of his head parallel to the wall. Although the positions of the silences were not very well defined, presumably in consequence of unequal amplitudes of the superposed vibrations, the most inattentive observer could not fail to notice that it was necessary to move the head considerably in order to pass from a place where the sound was a minimum for one ear to a place where it was a minimum for the other ear. We may therefore conclude that Savart's statement is not generally true, and that the views which he has founded upon it have no sufficient claim upon our acceptance.

When the median plane of the head is perpendicular to the wall, the silences are observed at distances which are *odd* multiples of the quarter wave-length, agreeably with theory.

Sensitive Flames.

The beautiful phenomenon of sensitive flames is now familiar to students of acoustics; but its rationale is by no means understood. An important contribution to the facts, from which some day a theory will doubtless arise, is contained in the observation of Prof. Tyndall as to the "seat of sensitiveness." My present purpose is to bring forward another fact which also will probably be found important. It may be thus stated. Under the action of stationary sonorous waves a flame is excited at *loops* and *not* at *nodes*.

The source of sound was a little contrivance on the principle of the bird-call, blown from a well-regulated bellows. Probably a very high organ-pipe or whistle might be employed; but it is necessary to use a nearly pure tone; and the pitch must be high, or the flame will be not affected sufficiently. At a distance of a few feet the sound was reflected perpendicularly from a large board. The flame itself was that called by Tyndall the vowel flame, issuing from a pin-hole steatite burner fed from a gas-holder with gas at high pressure (9 or 10 inches).

The observations were made by moving the burner to and fro in front of the board, until the positions were found in which the flame was least disturbed by the sound; and it will be seen from the results that these positions were very well defined. The distances between the board and the orifice of the burner was then taken with a pair of dividers, and measured on a scale of millimetres. Two observers, J and E, adjusted the flame independently of one another. The following are the results obtained:—

First position	{ J. $16\frac{1}{2}$, $16\frac{1}{2}$ mean $16\frac{1}{2}$ } E. 16, 16 " 16 }	$16\frac{1}{4}$	16.25
Second position.....	{ J. $31\frac{1}{2}$, $31\frac{1}{2}$ " $31\frac{1}{2}$ } E. $32\frac{1}{2}$, 31, 32 " $31\frac{3}{4}$ }	$31\frac{1}{2}$	15.7
Third position	{ J. $47\frac{1}{2}$, $47\frac{1}{2}$ " $47\frac{1}{2}$ } E. $46\frac{1}{2}$, 47, $45\frac{1}{2}$... " $46\frac{1}{4}$ }	$46\frac{3}{4}$	15.6
Fourth position ...	{ J. $62\frac{1}{2}$, $62\frac{1}{2}$ " $62\frac{1}{2}$ } E. 64, $60\frac{1}{2}$, $62\frac{1}{2}$... " $62\frac{1}{4}$ }	$62\frac{1}{4}$	15.6
Fifth position	{ J. $78\frac{1}{2}$, $78\frac{1}{2}$ " $78\frac{1}{2}$ }	$78\frac{1}{2}$	15.5

The second column contains the individual measurements by the two observers; the third shows the mean of all the results for the same position. The numbers in the fourth column are the results of dividing those of the third column by 1, 2, 3, 4, 5, respectively, and agree very well together, proving that the positions coincide with nodes. If the positions had coincided with loops, the numbers of the third column would have been in the ratios 1 : 3 : 5 : 7 : 9. The wave-length of the sound was thus 31.2 mm., corresponding to pitch f_{\sharp}^{vi} .

A few observations were made at the same time on the positions of the silences, as estimated by the ear listening through a tube. As was to be expected, they coincided with the loops, bisecting the intervals given by the flame. When the flame was in a position of minimum effect, and the free end of the tube was held close to the burner at an equal distance from the reflecting wall, the sound heard was a maximum, and diminished when the end of the tube was displaced a little in either direction. It may therefore be taken as established that the flame is affected where the ear would not be affected, and *vice versa*.

Aerial Vibrations of very Low Pitch maintained by Flames.

In a lecture "On the Explanation of certain Acoustical Phenomena,"* I showed the production of a pure tone of about 95 vibrations per second from a glass resonator and a

* Proceedings of the Royal Institution, March 15, 1875. Nature, vol. xviii. p. 319.

hydrogen-flame. With a larger resonator of the same kind—a globe with a short neck, intended for showing the combustion of phosphorus in oxygen, the pitch is 64 vibrations per second. I have lately made some further experiments, with the view of finding whether there is any obstacle to the maintenance by flames of vibrations of still lower frequency. The resonator, whose natural pitch is 64, was fitted with a paste-board tube 2 inches in diameter and 14 inches long. In this condition its calculated frequency* is about 25; and it was found that vibrations could be maintained by a hydrogen-flame, or even by a flame of common gas. The supply-tube should be of considerable length; and the orifice must not be much contracted. Although the intensity of vibration was such as to make it a matter of difficulty to keep the flame alight, scarcely any thing could be heard. I saw no reason to doubt that still slower vibrations might be maintained by flames.

In illustration of the mechanics of this subject, an apparatus was contrived, in which by the aid of electricity a periodic communication of heat to a limited mass of air could be effected. By means of a perforated cork one leg of a U-tube containing mercury was fitted air-tight to the neck of an inverted bottle of about 200 cub. centims. capacity. The diameter of the column of mercury was about 1 centim., and the length of the column about 25 centims. The combination constituted a resonator, differing from an ordinary air resonator by the substitution of mercury for air in the channel joining the interior of the vessel with the external atmosphere. Inside the bottle was a spiral of fine platinum wire, at one end in communication through the cork with one pole of a battery of two or three small Grove cells. The other end of the platinum spiral was connected with a copper wire, which terminated in the U-tube near the equilibrium-position of the mercury-surface. The second pole of the battery was in permanent connexion with the outer extremity of the mercury column. As the mercury vibrates, the circuit is periodically completed and broken. The current passes, and the platinum wire glows, when the mercury rises in the leg connected with the bottle. Thus the communication of heat occurs when the air in the interior is *condensed* by the vibration, which is the necessary condition for maintenance, as is explained in the lecture referred to.

Rijke's Notes on a large scale.

The production of sound in tubes by heated gauze was

* 'Theory of Sound,' vol. ii. § 307 (8).

discovered by Rijke*, and is perhaps the most interesting of all the cases in which vibrations are maintained by heat. The probable explanation is given in the Royal-Institution lecture. It is surprising that the phenomenon is not more generally known, as on a large scale the effect is extremely striking. I have employed a cast-iron pipe 5 feet long and $4\frac{3}{4}$ inches in diameter, hung over a table from a beam in the the roof of my laboratory. The gauze (iron wire) is of about 32 meshes to the linear inch, and may advantageously be used in two thicknesses. It should be moulded with a hammer on a circular wooden block of somewhat smaller diameter than that of the pipe, and will then retain its position in the pipe by friction. When it is desired to produce the sound, the gauze caps are pushed up the pipe to a distance of about a foot, and a gas-flame from a large rose-burner is adjusted underneath, at such a level as to heat the gauze to a bright red heat. For this purpose the vertical tube of the lamp should be prolonged, if necessary, by an additional length of brass tubing. In making the adjustment a more convenient view of the interior of the pipe is obtained with the aid of a small piece of looking-glass held obliquely underneath. Sometimes a sound is excited by the flame itself independently of the gauze. This should be avoided if possible, as it impedes the due heating of the gauze. When a good red heat is attained the flame is suddenly removed, either by withdrawing the lamp or by stopping the supply of gas. In about a second the sound begins, and presently rises to such intensity as to shake the room, after which it gradually dies away. The whole duration of the sound may be about 10 seconds.

Mutual Influence of Organ-Pipes nearly in unison.

The easiest way of approaching the consideration of this subject is to take the case of an open or stopped pipe, divided into two similar parts by a rigid barrier along its middle plane. In the absence of the barrier, the vibrations of the two halves under the action of the wind are in the same phase; and at first sight there appears to be no reason why this state of things should be disturbed by the barrier. Nevertheless it is well known to physicists that the two halves do in fact take opposite vibrations, with the result that the sound in the external air at a distance from the compound pipe is a small fraction only of that due to either half acting alone. In the pipe itself the vibration is *more*, and not less, intense on account of the barrier. It is true that at the very beginning of the sound,

* *Pogg. Ann.* cvii. 339, 1859.

when the wind first comes on, the vibrations in the two halves are similar, as is evidenced by the greater loudness ; but the opposition of phase is rapidly established, usually in a fraction of a second of time. As a system with two degrees of freedom, the compound pipe is capable of two distinct modes of vibration, in one of which the vibrations of the component pipes are in the same phase, and in the other in opposite phases. Why the action of the wind should maintain the latter mode of vibration to the exclusion of the former has not hitherto been explained ; but the fact remains that that mode of vibration, which depends for its possibility upon the barrier, is chosen in preference to the other mode, which is not dependent upon the barrier, and in the absence of the barrier is the one necessarily adopted.

The two possible modes of vibration have, as in almost all such cases, two distinct periods of vibration, the difference depending upon the behaviour of the air just outside the open ends. In consequence of the inertia of the external air at an open end, the effective length of a pipe exceeds its actual length by about six tenths of the radius. The increment of effective length is therefore greater in the case of the compound column of air when its parts vibrate in the same phase, than it would be for either of the parts if removed from the influence of the other. On the other hand, when the vibrations are in opposite phases, the increment must be much less, one component pipe absorbing the air discharged from the other. Accordingly one note of the compound pipe is graver, and the other, which is the one actually sounded, is more acute, than the natural notes of the component pipes when supposed to act independently of one another.

In order to show this effect it is not necessary that the two pipes be similar, or even of exactly the same pitch. If two pipes in approximate unison be placed so that their open ends are contiguous, a mutual influence is exerted, which is usually sufficient to prevent the production of beats. The examples about to be given will show that the unison need not be exact ; but the greater the deviation from unison, the more intense is the residual sound. Beyond the limit of the admissible departure from unison, beats ensue ; but at first they are irregular, and liable to be disturbed by very slight causes, such as draughts of air. According to theory, the frequency of the beats ought to be a little greater than the difference of the frequencies of the notes given by the pipes independently ; but I have not been able to detect the difference experimentally. It would therefore seem that over most of the range for which the mutual influence is sensible and

regular, it is sufficiently powerful to prevent more than one note being sounded.

In the experiments that I have tried, the pipes were blown from a bellows provided with a special regulator, and the pitches of the various notes were determined by counting the beats for 20 seconds between them and a somewhat sharper note on a harmonium. Sometimes the blown ends of the pipes were near together, and sometimes (in the case of open pipes) the unblown ends; but during the course of an experiment the positions of the pipes were not altered. In order to prevent a pipe speaking, I placed some cotton wool over the wind-way, and sometimes inserted a stopper; so that the pitch of the pipe as a resonator was entirely altered. The following are the details of some of the observations:—

I. Sept. 23. Open metal pipes about 2 feet long, one of them provided with an adjustable paper slider for modifying the pitch. Blown ends near one another; unblown ends distant.

Beats per second with harmonium-note.

One pipe alone.	Other pipe alone.	Both pipes together.
4·5, 4·5	5·0, 4·8	3·2, 3·1;

So that the note given by both pipes together is decidedly sharper than those of the separate pipes.

II. Sept. 23. Same pipes as in I. Unblown ends near one another; blown ends distant.

Beats per second with harmonium-note.

One pipe alone.	Other pipe alone.	Both pipes together.
4·8	5·1	3·8

III. Sept. 25. Same pipes placed parallel to one another at a distance of about 14 inches.

Beats per second with harmonium-note.

One pipe alone.	Other pipe alone.	Both pipes together.
5·15, 5·20	5·30, 5·45	5·00, 5·15

The note of both pipes together is somewhat higher than the notes of the single pipes.

IV. Sept. 26. Same pipes. Unblown ends near; blown ends distant.

Beats per second.

One pipe alone.	Other pipe alone.	Both pipes together.
5·80, 5·85	7·15, 7·50, 7·45	5·35, 5·50, 5·45

V. Sept. 26. Two bottles, tuned with water to about *g*, were blown by wind issuing from flattened tubes connected

with the bellows by lengths of india-rubber tubing. When the bottles were sufficiently removed from one another, the mutual influence was very small, being insufficient to prevent the formation of slow and pretty steady beats of about four seconds' period. This experiment shows that the mutual influence depends upon the proximity of the open ends of the pipes, and not upon any effects propagated through the supply-pipes leading from a common bellows.

Some further remarks on this subject will be found in a paper read before the Musical Association, Dec. 2, 1878. Reference may also be made to some allied experiments by Gripon*, with which I have only lately become acquainted. They appear scarcely to extend to the case with which I have principally occupied myself, namely that in which both pipes are blown. M. Gripon had, however, anticipated me in the experimental determination of the effect of a flange in modifying the correction for an open end† of a pipe.

Kettledrums.

The theory of the vibrations of uniform and uniformly stretched flexible circular membranes, vibrating *in vacuo*, has been known for many years‡. In practice deviations from such theoretical results are to be expected, if only in consequence of the reaction of the air, which must operate with considerable force on a vibrating body exposing so large a surface in proportion to its mass. In the case of kettledrums, the problem is further complicated by the action of the shell, which limits the motion of the air on one side of the membrane.

From the fact§ that kettledrums are struck, not in the centre, but at a point about midway between the centre and edge, we may infer that the vibrations which it is desired to excite are not of the symmetrical class. I find, indeed, that the sound undergoes little, if any, change when the central point is touched by the finger. Putting therefore the symmetrical vibrations out of account, we have to consider the parts played by vibrations of the following modes:—(1) that with one nodal diameter and no nodal circle; (2) that with two nodal diameters and no nodal circle; (3) that with three nodal diameters and no nodal circle; (4) that with one nodal diameter and one nodal circle, &c. The investigation proved to be of greater difficulty than I had expected, partly in consequence of the short duration of the sounds. Better ears than mine

* *Ann. d. Chim.* iii. p. 371, 1874.

† *Phil. Mag.* June 1877.

‡ 'Theory of Sound,' ch. ix.

§ De Pontigny. Proceedings of the Musical Association, Feb. 1876.

are liable to be puzzled in attempting to analyze compound sounds of such complication and irregularity. The following results, however, are believed to be trustworthy.

The principal tone corresponds to mode (1); the tone corresponding to (2) is about a *fifth* higher; that of mode (3) is about a major seventh above the principal tone; the tone of mode (4) is a little higher again, forming an imperfect octave with the principal tone. For the corresponding modes of a uniform membrane vibrating *in vacuo*, the theoretical intervals are those represented by the ratios 1·34, 1·66, 1·83, or about a fourth, a major sixth, and an interval nearly midway between a major and a minor seventh, respectively.

In experimenting on this subject it is important to bear in mind that the system of tones is really *double*, and that its components coincide only on the supposition of perfect symmetry. In practice the requirement of symmetry is difficult to attain even approximately; and thus it is that beats are generally heard, arising from the superposition of vibrations of nearly equal frequency. For the purpose of identifying the various modes, the want of symmetry is rather advantageous than otherwise. In the case of the gravest mode, I fastened with cement a small load (a halfpenny) to a point of the membrane situated about halfway between its centre and edge. In this way the two gravest tones fell asunder to about a semitone, one of them (the graver) being excited alone by a blow anywhere along the diameter through the load, the other alone by a blow anywhere along the perpendicular diameter. With the aid of a resonator tuned to the pitch of the *subordinate* tone, the nodal diameters of the two modes (1) may be fixed with great precision by the absence of beats. With a resonator turned to a pitch midway between those of the two tones, the beats are most distinct when the blow is delivered at a point near the middle of one of the four quadrants formed by the two nodal diameters; but the position for the most distinct beats necessary varies with the pitch of the resonator, and also with the situation of the observer. It may be remarked that, provided the deviation from symmetry be moderate, the same vibrations (except as to phase) are excited, whether a blow be delivered at any point, or at the other point on the same diameter equally distant from the centre; and vibrations excited by striking one point are damped by touching the other. The other modes with nodal diameters only were identified in a similar way. The mode (4) with a nodal circle is known by the cessation of sound at a particular point when various places along a radius are tried; on either side of this point the sound revives.

The drum that I examined is of about 25 inches diameter; and

the form of the shell is nearly hemispherical. During the experiments the pitch of the principal tone was about 120 vibrations per second. The vibrations were excited by a small wooden hammer, such as is used for harmonicons, the head being covered with cotton-wool tied on with string. For the graver tones the thickness of the cotton wool may with advantage be greater than for higher tones.

I am not in a position to decide the question as to the function of the shell; but I think it at least doubtful whether it introduces any really advantageous modification into the relations of the component tones. It is possible that its advantage lies rather in obstructing the flow that would otherwise take place round the edge of the membrane. It must be remembered that the sounds due to the various parts of a vibrating membrane interfere greatly. In the case of a membrane simply stretched upon a hoop, and vibrating away from all obstacles, no sound at all would be heard at points in the prolongation of its plane. And even when there is a shell, no sound would be heard at points on the axis of symmetry, at least if the symmetrical vibrations may be left out of account.

The Æolian Harp.

So far as I am aware, it has always been assumed by writers who refer to this subject that the vibrations of the string are in the plane parallel to the direction of the wind; and, indeed, the action of the wind in maintaining the motion is usually explained as the result of friction, and as analogous to the action of a violin-bow. It is more than a year since I made some experiments with the view of testing a suspicion of the incorrectness of this view; and I then arrived at the conclusion that the vibrations are in fact executed in the plane perpendicular to the direction of the wind. I suppose for simplicity that the length of the string is perpendicular to the direction of the wind, as is usually the case in practice. Recently I have repeated these experiments in an improved form, and with confirmatory results.

The best draught is that obtained from a chimney. In my later experiments a fireplace was fitted with a structure of wood and paper, which could prevent all access of air to the chimney, except through an elongated horizontal aperture in the front (vertical) wall. The length of the aperture was 26 inches, and the width 4 inches; and along its middle a gut string was stretched over bridges. The strength of the draught could be regulated by slightly withdrawing the framework from the fireplace, so as to allow the passage of air to the chimney otherwise than through the slit.

A fine point of light was obtained from a fragment of a silvered bead attached to the string with wax, and illuminated by a suitably placed candle, and was observed in the direction of the length of the string through an extemporized telescope. In this way there could be no mistake as to the actual plane of vibration, or uncertainty as to the direction of the wind over the string. The path of the point of light was seen to be nearly rectilinear and *vertical*, showing that the vibration is *across* the wind. Sometimes the path was sensibly elliptic with the major axis vertical.

When a string is stretched across the slit at the bottom of a slightly open window, there is usually some difficulty in determining the actual direction of the wind where it plays upon the string. On a still night, and with a regular fire, the sound is sometimes steady for a long time, but it is wonderfully sensitive to the slightest changes in the draught. On one occasion it was found impossible to open a distant door so slightly as not to stop the sound, which would revive in a few seconds after the door was closed again. A piece of paper no larger than the hand thrown upon the fire (which was burning without flame) altered the draught sufficiently to stop the sound until the heated air due to its combustion had passed up the chimney. It is the irregularity, and not, as has been asserted, the insufficient intensity, of the wind which prevents the satisfactory performance of the harp in the open air.

Terling Place, Witham,
Feb. 8, 1879.

XXVIII. *On High Electrical Resistances.*

*By J. HOPKINSON, F.R.S., D.Sc.**

IN the Philosophical Magazine of July 1870 Mr. Phillips describes a method of readily constructing very high electrical resistances. A pencil-line is ruled on glass; the ends of the line are provided with the means of making electrical connexion; and the whole is varnished: by this means a resistance of two million ohms was obtained; and it was found to be constant under varying potential. This method of constructing resistances is alluded to in Maxwell's 'Electricity' (p. 392); but I do not know that it has received the examination it deserves, or that it has come into general use. Having need of resistances of over 100 million ohms, I have made a few on Mr. Phillips's plan, ranging from 26,000 ohms to 96,000,000 ohms (which are fairly satisfactory), and one or two

* Communicated by the Author.

much greater (which do not conduct according to Ohm's law, but with a resistance diminishing as the electromotive force increases). A short description of these may perhaps save a little trouble to others who desire tolerably constant high resistances.

All my resistances are ruled on strips of patent plate glass which has been finished with fine emery, but has not been polished. The strips are twelve inches long, and, except in the cases specified below, about half an inch wide. One or more parallel lines are ruled on each strip, terminating at either end in a small area covered with graphite from the pencil. The strip of glass, first heated over a spirit-lamp, is varnished with shellac varnish, excepting only these small terminal areas, which are surrounded by a small cup of paraffin-wax to contain mercury to make the necessary connexions. To secure better insulation, feet of paraffin or of glass covered with paraffin are attached on the underside at the ends of the strip to support it from the table. Before varnishing, each strip was marked with a distinguishing letter. The strips marked *g*, *h*, *i*, *a*, and *b* were ruled with a BB pencil, the remainder with a HHH.

These resistances appear to be not quite constant, but to vary slightly with time, the maximum variation in four months being slightly in excess of $\frac{1}{2}$ per cent. In every case they were examined under varying potential to ascertain if they obeyed Ohm's law. With the exception of *f*, described below, all were satisfactory in this respect. The resistance appears to diminish slightly as the temperature rises; but this conclusion rests on a single rough experiment, and must be regarded as uncertain.

The values of the resistances were determined with a differential galvanometer, each coil having a resistance of 3500 ohms, by the well-known method of dividing a battery-current, passing one part through the large resistance to be measured and one coil of the galvanometer, the other through a set of coils or other known resistance, and then through the galvanometer shunted with a second set of resistance-coils. *g* was thus compared with standard coils. *g* was then used to find *h* and *i*; and *h* + *i* was used to find *a* and *b*. A Thomson's quadrant electrometer was used to compare in succession *k*, *l*, and *m* with *a* + *b*. *c* and *e* were similarly compared with *k* + *l* + *m*; and, lastly, *c* and *e* were used to examine *f*.

g is ruled on a strip one inch wide, rather more than half the surface being covered with graphite. Three experiments on the same day gave 26,477, 26,461, and 26,470 ohms; the variations are probably due to uncertainty in the temperature-correction, the galvanometer-coils being of copper. After the lapse of four months 26,615 ohms was obtained.

i is ruled on a strip three quarters of an inch wide, with nine

tolerably strong lines; its resistance was first found to be 209,907 ohms, and four months later to be 208,840.

b has four strong lines on a strip half an inch wide; resistance 207,954 on a first occasion, and 208,750 after the lapse of four months.

a has two lines narrower than the preceding; resistance 5,240,000 at first, and 5,220,800 after four months.

h has a single line apparently similar to either of those of *a*; and the resistance is 9,168,000.

k, *l*, and *m* have each two lines ruled with a HHH pencil; their resistances are respectively 23,024,000, 14,400,000, and 13,218,000 ohms.

c and *e* also have two lines, but they are finer; the resistances are 79,407,000 and 96,270,000.

As already mentioned, all the preceding were tested with various battery-power, and were found to obey Ohm's law within the limits of observation. It was not so with *f*, as the following observation shows very clearly. *c*, *k*, *e*, and *f* were arranged as a Wheatstone's bridge. Junctions (*f*, *c*) and (*e*, *k*) were connected to the poles of a Daniell's battery varying from one to eighteen elements; junctions (*e*, *f*) and (*k*, *c*) were respectively connected through the reversing-key with the quadrants of the electrometer. The potential of one Daniell's element was represented by 270 divisions of the scale of the electrometer. Column I. gives the number of elements employed, II. the corresponding reading of the electrometer, III. the value of $\frac{k}{k+c} - \frac{e}{f+e}$ deduced therefrom, and IV. the values of the ratio resistance of *f*: resistance of *e*.

I.	II.	III.	IV.
1	16	0.060	5.1
2	25	0.046	4.6
3	31½	0.039	4.4
4	31	0.029	4.1
5	28	0.021	3.9
6	27½	0.017	3.8
9	10	0.0041	3.5
12	— 5	—0.0016	3.4
15	—25	—0.006	3.3
18	—47	—0.0097	3.25

This result is by no means surprising. There is doubtless an exceedingly minute discontinuity in the fine line across which disruptive discharge occurs; and the moral is, that resistances of this kind should always be tested as regards their behaviour under varying electromotive force.

Several attempts to rule a line on a strip 12 inches long with a resistance over 100,000,000 ohms resulted in failure.

XXIX. *Methods of Measuring Electric Currents of great Strength; together with a Comparison of the Wilde, the Gramme, and the Siemens Machines.* By JOHN TROWBRIDGE, *Harvard University**.

[Plate VIII. figs. 1 & 2.]

THE measurement of electric currents of great strength can be classed under four heads:—No. 1. The Galvanometric method; No. 2. The Electrometer Method; No. 3. The Heat Method; No. 4. The Electrodynamometer Method.

No. 1. *The Galvanometric Method.*

With a galvanometer of small resistance and of large radius, it is necessary to bring the deflection to the neighbourhood of 45° by means of a shunt of very small resistance. The errors increase when the deflections exceed 45° in a divided circuit; and by the use of a shunt of small resistance, any error in the measurement of this small resistance multiplies the whole observation by this error.

By the use of a cosine galvanometer which I devised in 1871, and published in the 'American Journal of Science' for that year, the use of shunts can be modified; but there are difficulties, from the dip of the needle and from want of accuracy in graduations of the circle which measures the deflection of the moving coil from the vertical plane.

In practice it is very inconvenient to find a suitable shunt which will answer for a wide range of experiments, and different shunts have to be used. Moreover the heating of the shunt multiplies the observations by an error. In short, by the use of a shunt method, we measure a large quantity by observations upon a hundredth or a thousandth part of itself, and proceed from a small quantity to a large one, which is a fundamentally defective method.

No. 2. *The Electrometer Method.*

By means of a suitable electrometer, the difference of potential of two points in a closed circuit can be measured; and from this the electromotive force in volts can be estimated. The difficulty of dealing with static electricity in electrical measurements is well known. Leakage, want of constancy of charge in the electrometer, nay, impossibility of maintaining a charge in certain localities, limit the use of this method, even if the results obtained were not approximate.

* From the Proceedings of the American Academy of Arts and Sciences, New Series, vol. vi. pp. 122-132. Communicated by the Author.

No. 3. *Heat Method.*

By the use of the law that the heat developed in a circuit is expressed by $H = C^2 R t$, where C is current in Webers, R = resistance, t = time, we can deduce C by measuring the rise of temperature of a given volume of water. Measurements of temperature are especially fraught with difficulties on account of conduction, radiation, and errors of thermometers, beside consuming time in waiting for the proper conditions for a given experiment.

No. 4. *The Electrodynamometer Method.*

The principle of Weber's electrodynamometer is well known. The electric current passes down one wire of the bifilar suspension of a movable coil and up the other, and then through fixed coils surrounding the movable coil. Maxwell, in his 'Electricity and Magnetism,' vol. ii. p. 332, remarks:—"Weber's form of the electrodynamometer, in which one coil is suspended within another, and is acted on by a couple tending to turn it about a vertical axis, is probably the best fitted for absolute measurements." With powerful currents, however, it is necessary to shunt this instrument, and the errors inherent in this method are introduced. Even with moderate currents, the directive force of the bifilar suspension is changed by the elongation of the wire from a rise in temperature. If we keep within the point at which the wires are elongated, the deflections are slight and subject to error of observation.

In working with dynamo-electric machines, it is important that we should avoid the method of shunts; for the entire resistance of the circuit is generally of the same order of magnitude as the shunts employed. It is necessary that we measure the whole strength of the current directly at the same time that we measure the work consumed in driving the dynamo-electric machine, the velocity of the machine, and the resistance of the circuit. It is also important to eliminate local attractions. The time consumed in measuring the current-strength should be small.

The instrument described in this paper fulfilled the conditions prescribed.

Fig. 1 (Pl. VIII.) shows the general aspect of the apparatus. The large fixed coils were made of copper bands, 35 millims. broad and 1 millim. thick. There were twelve coils, six on each side of the movable coil, which is shown with its suspension between them. The large coils were reinsulated from each other by vulcanite washers, and held together by brass rivets insulated by vulcanite cylinders. The coils were placed at a distance apart equal to their thickness, and thus allowed cur-

rents of air to pass freely between them. This arrangement is shown at I (fig. 2). The bifilar suspension is connected with a graduated circle which reads by means of verniers to one minute. The tangent and clamping-screws of the torsion-head are not shown in the figure. The general arrangement was similar to that used by Mr. Latimer Clark, and figured in Maxwell's 'Electricity,' with the exception that the graduation was not upon a cylinder, but was on a plane, and the graduated circle was such as is used on spectrometers. The torsion-head admitted of vertical adjustment by means of the hollow cylinders at its point of support, in addition to the vertical adjustment of the pulley by means of which the tension upon the suspending-threads was equalized. In the ordinary form of electro-dynamometer the current passes down one suspending-wire and up the other. In my dynamometer this is not the case, as is shown in fig. 2. Therefore the suspension can be made of strands of silk or any suitable material, according to the sensitiveness desired. In the actual use of the instrument with powerful current, it was found necessary to use steel wire, in order to increase the directive force, so great were the deflections.

The movable parts are best shown in fig. 2. The construction of the central coil is shown at D. The water enters at *a*, passes out at *a* after cooling the hollow chamber B, which admits of adjustment, and then flows by rubber tubing to *f*, and, after cooling the mercury-cup E, flows out through *f*. G is the water-chamber which answers to B. At *n*, below the mirror *m*, is a bar upon which are hung cylindrical weights to determine the moment of inertia to alter the sensitiveness. Only one coil and a half are shown in the figure. The electric current enters at H, passes through the mercury-cup to E, then to C, and thence by the hollow cup to O, and then around the outer coils.

A telescope with scale was employed to read the deflections; but it was found better, in practice, to use the graduated circle of the torsion-head and bring the movable coil back to zero. In this case we have, from the theory of the electro-dynamometer,

$$C^2 = \sqrt{\frac{F}{Gg}} \sin \theta;$$

and the effect of the earth and local attraction are eliminated. By this method of observation no telescope and scale are needed: it is only necessary to bring the point of the bar which passes through the movable coil to a fixed point. The mercury in the pivot-cups serves to damp the vibrations of

the movable part of the apparatus; and it was found that readings could be taken quicker than by galvanometric methods.

Theory of Instrument.*

$$C^2 = \frac{F \tan \theta}{Gg \cos \beta};$$

where

C = current,

F = directive force,

G and g = constants of fixed and movable coils,

β = angle of coil with magnetic meridian.

If the torsion-head of the instrument can be adjusted so that the deflection is zero, and $\theta = -\beta$, we have

$$C^2 = \frac{F}{Gg} \sin \beta.$$

The value of F was determined by several methods. Since

$$Ft^2 = \pi^2 A,$$

where

t is the time of vibration,

and

A = moment of inertia,

it is necessary to determine both the time of swing and the inertia. The times of swinging were obtained by means of a chronograph upon which seconds were recorded by the side of the records produced by breaking an electric circuit at the instant the movable coil passed the middle of its vibration. The moment of inertia was first determined experimentally by adding known cylindrical weights and determining the new time of vibration.

We thus have

$$A = k \left(\frac{l^2}{\left(1 + \frac{w}{w_1} t_1^2 - t^2 \right)} \right)$$

and

$$k = w \left(l^2 + \frac{r^2}{2} \right),$$

where k = moment of inertia of added cylindrical weights, w = weight of cylinders in milligrammes, l = distance of point of suspension of cylinders from axis, r = radius of cy-

* Maxwell's 'Electricity,' vol. ii. p. 329.

linders, and w_1 mass of moving parts before w was added ; the dimensions being in millimetres.

From these expressions we obtain

$$F = \frac{\pi^2 k}{\left(1 + \frac{w}{w_1}\right) t_1^2 - t^2}.$$

The constants G and g were calculated from the actual measurements of the coils, which could be made with great accuracy, since all the parts were large.

The constants were as follows :—

$$\text{mean radius } r = 153.3 \text{ millims.}$$

$$Gg = 1631.45 \quad ,,$$

$$\frac{F}{Gg} = 656.626 \quad ,,$$

The constant was also determined by running the same current through the electro-dynamometer and a tangent galvanometer of one turn of copper wire, whose radius was r , and whose constant was equal to $\frac{rT}{2n\pi}$.

In this case

$$C^2 = \frac{r^2 T^2}{4n^2 \pi^2} \tan^2 \theta = \frac{F}{Gg} \sin \phi,$$

and

$$\frac{F}{Gg} = \frac{r^2 T^2}{4n^2 \pi^4} \cdot \frac{\tan^2 \theta}{\sin \phi};$$

where

T = horizontal force of earth's magnetism,

r = radius of galvanometer-coil,

θ = deflection of galvanometer,

ϕ = deflection of electro-dynamometer.

The result obtained in this way closely agreed with that obtained by the previous method.

No difficulty was experienced from the heating produced by currents of even eighty vebers when the current was allowed to run for a long period through the instrument : as long as the stream of water was maintained around the mercury-cups, even a small immersion of the point of the axis of the movable part of the instrument did not result in heating. By this instrument, therefore, the whole current could be measured without shunting. At first the metal pivots which

dipped in the mercury were tipped with aluminium; but when a strong current passed through them the mercury was disturbed by an apparent ebullition, and the mercury was speedily covered with a black deposit. It was found that copper points would answer perfectly well. Distilled mercury was used in all cases; it answered the double purpose of conducting the current and bringing the vibrations quickly to rest.

Through the courtesy of Captain Breese, U.S.N., in charge of the U.S. Torpedo Station at Newport, R. I., who obtained permission for me to use the dynamo-electric engines at that place, I was enabled to make a series of measurements with the dynamometer described above.

The resistances used consisted of large bands of German silver, each in the neighbourhood of $\frac{1}{10}$ of an ohm resistance. The foot-pounds of work consumed were measured by a Batchelder's dynamometer*, which is fully described in Dingler's 'Polytechnic Journal,' 1844, vol. ii. This dynamometer is not suitable for the measurement of small or great horse-power; but it answered very well in the limits of velocities and horse-powers to which I confined myself. An accurate measure of the work consumed in running a dynamo-electric machine upon a closed circuit would require the use of gearing instead of belting; for it is difficult to estimate the slip of the belting. On account of the error introduced from this latter-mentioned cause, I have given the whole work required to run each machine on a closed circuit. The slip on an open circuit would be small, but on a closed circuit might be very large. The machines were run under the same conditions of shafting and pulleys. It was estimated that the Siemens required 0.031 horse-power on an open circuit, and the Gramme 0.206 to 0.328 horse-power. The term efficiency denotes the ratio of the equivalent in metre-grammes of the current produced to the metre-grammes consumed in running the dynamo-electric machine. Since one veber through one ohm

$$= C = \frac{10^5}{10^7} = 10^{-2},$$

the work $w = C^2 R t = (10^{-2})^2 \times 10^7 = 10^3 = 1000$ units of work; and dividing by the unit employed, we have

equivalent of 1 veber = 102 metre-grammes,

1 foot-pound = 138 „

* For which I am indebted to the Massachusetts Institute of Technology.

Wilde Machine (large size).

Resistance of circuit, in ohms.	Current, in webers per second.	Speed of machine, per minute.	Metre-grammes consumed per second.	Equivalent of current in metre-grammes per second.
·594	62·33	548	350·658	235·480
·733	61·76	508	392·403	285·293
·857	43·82	532	283·107	167·907
·907	60·25	500	453·123	335·966
1·039	39·28	520	298·356	163·682
1·120	43·44	548	343·827	215·660
1·241	50·43	504	542·685	322·047
1·453	44·94	520	553·311	309·658
1·593	47·51	536	633·765	366·910
2·305	32·86	528	643·632	253·968

The measurements with the Wilde machine were made with an electro-dynamometer similar to that described in Maxwell's 'Electricity and Magnetism.' It was constructed on the Helmholtz-Gauguin principle, and had a resistance of 58·9 ohms. A shunt of 1 ohm had to be employed; and the instrument was also coupled in multiple arc to avoid the lengthening of the bifilar suspension.

Gramme Machine (large size).				
Resistance of circuit, in ohms.	Current, in webers per second.	Number of revolutions of armature per minute.	Metre-grammes consumed per second.	Equivalent of current in metre-grammes per second.
·675	86·0	432	589·743	509·418
·760	75·6	462	534·336	442·211
·781	75·6	452	607·200	455·377
Siemens Machine (large size).				
·973	79·8	264	831·105	632·255
1·055	68·8	294·5	743·820	509·569
1·066	66·0	325	839·454	472·805

I add a few data in regard to the dimensions of these machines, which are partly taken from the reports of the Secretary of the Navy for 1877, and partly from the Station records, which were generously placed at my disposal.

The Gramme Machine.—This machine weighs about 2700 pounds, stands 30'' high, is 40'' long and 34'' wide. It is driven by a pulley 15'' in diameter. The armature moves with very little friction. The field-of-force coils are flat; and

there are four of these, each about 10" long, $3\frac{3}{4}$ " deep, and 22" wide. The armature resistance is 0·129 ohm, the field resistance 0·212 ohm, thus making ·341 ohm for the total internal resistance. The total weight of wire in the machine is 483 pounds; or the weight of wire is nearly 18 per cent. of the total weight of the machine.

Siemens Machine, or Heffner-von-Altenek Machine, built by the Siemens Brothers.—This machine is 61" in length, 28" in breadth, and 12" in height. The armature is nearly 34" long and $9\frac{1}{2}$ " external diameter. It is formed by winding 98 pounds of two insulated wires longitudinally, and in eight divisions, around a thin and hollow brass cylinder. Within this hollow cylinder is a hollow stationary cylinder of cast iron, supported by bearings that pass through the brass cylinder. The commutator has eight divisions, which are eight sector-shaped sheets of brass, insulated from but attached to the face of a plate which is outside of one of the bearings of the brass cylinder. Two collectors or brushes trail upon and press against these sectors: these brushes have a bearing so extensive as to short-circuit or bridge over the edge of two sectors. The spark of the commutator is quite insignificant. This machine differs from all others in this respect—the armature simply moves a wire through a field of force, and not a soft-iron core covered with wire. The resistance of the entire circuit, field of force, corrected for conductivity, is ·586 ohm. The normal velocity of the machine is from 370 to 380 revolutions per minute.

Wilde Machine.—This resembles in some respects the Hjorth machine of 1855 with the permanent magnet omitted. It has two armature circuits—one with current uniform in direction for the purpose of maintaining the magnetism of the field, and the other for producing the electric light. The current from this last circuit is a to-and-fro current, without commutator.

"The armature wire weighs 28 pounds, and is divided into two circuits: about 7 pounds of it, having a resistance of ·454 ohm, furnishes the current which maintains the field. The remainder (21 pounds), having a resistance of ·074 ohm, maintains the to-and-fro current. About 325 pounds of wire are distributed in 24 coils to make up the electromagnetic field, which has a resistance of 2·83 ohms. These coils are $10\frac{1}{4}$ " in length and $3\frac{7}{8}$ " in external diameter, having soft round cores 2" in diameter. There are 24 armature cores and coils, one half on each side of a central cast-iron wheel $1\frac{1}{2}$ " thick. The central diameter of this wheel is 18" nearly. The whole weight of wire in this machine is nearly 354 pounds." The normal velocity of the machine is about 600 revolutions. A greater

rate of speed would have increased, to a certain extent, the currents produced by the Siemens and the Gramme machines ; on the other hand, more horse-power would have been necessary to attain this increased speed. The Wilde machine requires more horse-power to run it as the resistance of the outside circuit increases. This is due to the construction of the machine, and is not the case with the Siemens and the Gramme machine. A certain proportion between the resistance of the machine and that of the outside circuit is undoubtedly best for greatest efficiency of dynamoelectric machines ; and a certain velocity is necessary to attain the greatest efficiency.

From my experiments, I should class the machines as follows:—

Gramme,
Siemens,
Wilde.

Theoretically the Siemens machine should give the best results. At the time of my experiments the Siemens machine suffered the disadvantage of being run at a less rate of speed than the other machines.

I hope to pursue these tests under conditions resulting from higher speed. Generally speaking, that machine is the best which gives the greatest efficiency at low rates of speed ; for the necessity of high speed introduces much that is detrimental to the locality of the machine and to the machine itself. At the present time alternating machines are coming into notice again, in connexion with electric lighting ; and a suitable electro-dynamometer is desirable in the measurement of the current produced by these machines. The instrument which I have tested above seems to fulfil the proper conditions.

My thanks are due to the officers of the station for their generous assistance and free disposal of the resources of their electrical department.

XXX. *A Proof of the Electromagnetic Rotation of the Plane of Polarization of Light in the Vapour of Sulphide of Carbon.*
By A. KUNDT and W. C. RÖNTGEN*.

[Plate VIII. fig. 3.]

FARADAY, as is well known, did not succeed in demonstrating the electromagnetic rotation of the plane of polarization of light in gases ; and since his time that a rotation has not been observed.

* Translated from a separate impression, with an original postscript, communicated by the Authors.

On account of the interest presented by the question whether this property does not belong to gases in general, we resolved to repeat the experiments once more with the most powerful currents and, in all other respects, under the most favourable conditions; and we have now succeeded in verifying the phenomenon sought, at least for the vapour of bisulphide of carbon.

We selected this substance for the experiments because, on the one hand, in the liquid state it exhibits an energetic electromagnetic rotation, and, on the other, its vapour possesses considerable tension even at comparatively low temperatures.

The apparatus employed for enclosing and heating the carbon bisulphide is delineated in fig. 3, Pl. VIII., one tenth of its actual size. An iron tube, *aa*, is furnished at its ends with two strong conically out-turned brass collars, *bb*; into these two brass pieces, *cc*, likewise conical, can be inserted, and each pressed in firmly by means of six strong screws. The inserted pieces are perforated in the direction of the length of the tube (diameter of the perforations 1 centim.); and to the side turned towards the interior of the tube two glass plates, *dd*, 1 centim. thick, are cemented, which are also held by strong screws. To the insertion-pieces, *c*, two tin-plate tubes, *ee*, are screwed; and the whole is enclosed in the tin-plate tube *ff*, in the centre of which it is held by the two corks *gg*. The tubes *ee* stand out about a centimetre beyond the corks. By an inlet-tube, *h*, in one of the corks, steam can be introduced into the space between the iron tube and the surrounding tin-plate tube; through a tube, *i*, in the other cork it can pass out again. Consequently the iron tube could, by steam led round it, be heated in its entire length to 100°. The outer tin-plate tube was enclosed in six large coils of wire.

The wire was of 3 millims. thickness.; in each coil there were about 400 turns, through which the current of 64 large Bunsen cells could be sent.

For the experiment some bisulphide of carbon was poured into the iron tube, and the air expelled by the carbon-bisulphide vapour already forming at the ordinary temperature. Then the inset pieces were screwed as fast as possible to the ends, the tube with its appendages was fixed in its place inside the wider tin-plate tube and the spirals, and steam introduced. As soon as the entire tube had acquired the temperature of boiling water, all the mist that had appeared on the glass plates during the heating vanished, and the plates and the sulphide-of-carbon vapour that had formed in the tube were perfectly transparent. A rectilineal pencil of light, polarized

by a Nicol, was now sent through the vapour; a Nicol at the other end extinguished the pencil. The current of the 64 elements was now sent through the coils, when the field was distinctly brightened. The brightening was still more considerable when, after the closing of the current, the front Nicol was rotated to darkness and the current then reversed by a commutator.

The rotation of the polarization-plane took place, as was to be expected, in the direction in which the positive current went through the coils.

In order to find out whether the observed rotation might not be entirely or in part produced by the glass plates that closed the ends of the tube, the sulphide-of-carbon vapour was expelled, and the empty tube again heated and observed. On closing the current there was in fact shown a very slight rotation proceeding from the glass plates; but its amount was essentially less than when the experiment was made with sulphide of carbon in the tube. In order to get quite rid of this feeble glass-plate rotation, the two outermost coils (those nearest to the plates) were put out of the circuit, and the four through which the current still passed were now removed so far from the glass plates that their influence upon the latter could only be extremely slight. It was in fact now found that the empty tube, heated by steam, showed no trace of rotation. But when the tube was again filled with sulphide-of-carbon vapour, at the closing of the current through the four coils a distinct brightening of the field of vision, previously darkened by crossing the Nicol, was obtained. We could not measure precisely the amount of the rotation: we estimated it in the last experiment at about $\frac{1}{2}^{\circ}$.

By this it is demonstrated that saturated vapour of bisulphide of carbon, at about 100° C., rotates the plane of polarization of light in the magnetic field.

When some sulphuric ether was put into the iron tube and heated, on closing the current no rotation could be observed.

Although, it is true, by our experiments it has at present been shown only that saturated bisulphide of carbon exhibits electromagnetic rotation of the plane of polarization, yet it is now hardly to be doubted that it will be possible to prove the existence of the polarization in unsaturated vapours and gases also. We are engaged in the construction of an apparatus which shall permit us to examine permanent gases at very high pressures in the magnetic field, in order to demonstrate the rotation for these, and, if possible, to follow up the phenomenon with measurements.

It will be especially interesting to ascertain whether oxygen

rotates the plane of polarization in the same direction as the other gases.

Strasburg, October 1878.

Postscript.—Since the publication of the foregoing experiments we have further improved the apparatus employed, by giving to the iron tube (*aa* in the figure) a length of 2.4 metres. The glass plates *dd* were now so far from the ends of the six coils that, on closing a current from 70 large Bunsen elements, they did not produce any perceptible rotation. A repetition of the experiments with bisulphide-of-carbon vapour gave now also an evident rotation of the plane of polarization.

In like manner we succeeded in observing the electromagnetic rotation in gaseous sulphurous acid at 100° C. and a pressure of about twenty atmospheres, and in sulphuretted hydrogen gas at the ordinary temperature and about twenty atmospheres.

In air, up to twenty-five atmospheres, we have not yet been able to observe any rotation. We will not omit to remark that, apart from employing very high pressures, another way presents itself of making the observations for the investigation of the rotation in air—with polarized sunlight, which with the aid of heliotropes in the direction of the earth's magnetic meridian is sent through a long stretch of the atmosphere.

XXXI. *Note on the Analysis of the Rhombohedral System.*

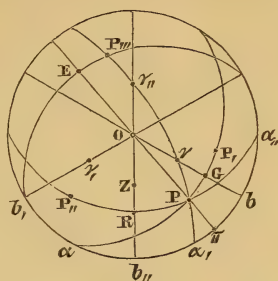
By W. J. LEWIS, M.A., *Fellow of Oriel College, Oxford*.*

THE methods followed by Professor Miller and most writers for obtaining the formulæ employed in determining the indices of a form in the rhombohedral system from the measured angles, or, conversely, the angles from the given indices, are, though elegant, difficult and perplexing. It occurred to me that they might be easily obtained by means of the anharmonic ratio of four poles in a zone applied to three known poles in one of the planes of symmetry, and a fourth pole whose position and indices can be directly connected with the poles of the form to be determined. This method brings out in a prominent manner the relation (2); a relation to be found in all the books, but so disguised and so little noticed as easily to be passed over, whereas from its simplicity, and from the fact that the angle involved in it is the first deduced from the

* Communicated by the Crystallological Society. Read Nov. 24, 1878.

measured angles of a scalenohedron, it contains a smaller error than any other equation.

The figure represents the stereographic projection of some of the principal poles and planes of a rhombohedral crystal, together with the poles P of a form $\{hkl\}$ to be determined. The poles r are $\{100\}$, $o(111)$; therefore the poles b and a are $\{2\bar{1}1\}$ and $\{01\bar{1}\}$ respectively. Let P be (hkl) , P_r , P_{rr} the corresponding faces repeated over O b and O b_{rr} . Then P_r is $(h\bar{l}k)$, and P_{rr} $(k\bar{h}l)$. Let Q , R , π be the intersections of the pairs of zones $[PP_r]$ $[Ob]$, $[PP_{rr}]$ $[Ob_{rr}]$, $[OP]$ $[bb_{rr}]$ respectively. Then the indices of Q are $(2h, k+l, k+l)$, of R $(h+k, h+k, 2l)$, and of π $(2h-k-l, -h+2k-l, -h-k+2l)$.



The anharmonic ratio of the poles a , b_{rr} , π , b gives

$$\frac{\sin b\pi}{\sin \pi a} : \frac{\sin bb_{rr}}{\sin b_{rr}a} = \left[\frac{b\pi}{\pi a} \right] : \left[\frac{bb_{rr}}{b_{rr}a} \right] = \frac{k-l}{2h-k-l}$$

(Miller's 'Treatise on Crystallography,' p. 14). Hence

$$\tan b\pi = \tan XOP = \frac{(k-l)\sqrt{3}}{2h-k-l} \quad \dots \quad (1)$$

The anharmonic ratio of the poles O , r , Q , b gives, in a similar manner,

$$\frac{\tan OQ}{\tan Or} = \left[\frac{OQ}{bQ} \right] : \left[\frac{Or}{br} \right] = \frac{2h-k-l}{2(h+k+l)};$$

and writing D for the element Or , we have

$$\tan OQ = \frac{2h-k-l}{2(h+k+l)} \tan D. \quad \dots \quad (2)$$

Similarly from the poles O , R , b_{rr} , and $z(22\bar{1})$ the dirhomboidal face of r_{rr} we obtain

$$\frac{\tan OR}{\tan D} = \left[\frac{RO}{Rb_{rr}} \right] : \left[\frac{zO}{zb_{rr}} \right] = \frac{h+k-2l}{2(h+k+l)} \dots \quad (2')$$

From the right-angled triangle POQ we have

$$\tan OP = \tan OQ \sec b\pi; \quad \dots \quad (A)$$

\therefore from (1) and (2),

$$\tan^2 OP = \frac{(k-l)^2 + (l-h)^2 + (h-k)^2}{2(h+k+l)^2} \tan^2 D. \quad \dots \quad (3)$$

The equation (2) is given by Professor Miller in his 'Treatise on Crystallography,' 1839, in the form

$$\tan PO \tan XO \cos XOP = \frac{2h-k-l}{h+k+l},$$

and in the equivalent form

$$2 \tan PO \cot OA \cos XOP = \frac{2h-k-l}{h+k+l},$$

the latter being the same as (2), with the sole difference that $\tan OQ$ is replaced by its value given by equation (A). The form in which it is given by Professor Miller does not, however, bring out so prominently the simplicity and directness of the relation existing between the quantities involved in the equation and those given by observation.

As an illustration of the utility of equations (2) and (2'), let us take the determination of a scalenohedron on a mineral (such as calcite) whose elements are known. Measurement of two of the angles between adjacent faces suffices for the determination. If PP_1 and PP_{11} are the two angles measured, we know the three sides of the triangle aPa_{11} ; and the angle

$Pab = bQ = \frac{\pi}{2} - OQ$ is the first quantity deduced from the

measurements. Equation (2) then gives a simple equation in terms of the indices h, k, l . If PP_1 or PP_{11} be given with the angle of the middle edge of the scalenohedron, we know the sides of the triangles aPa or $a_{11}Pa_{11}$. In the first case OQ is determined as before, in the second OR ; and we must employ (2) or (2') accordingly.

To complete the analysis, I need only point out that the relations connecting the indices of dirhombohedral forms can be most simply obtained by aid of the equations connecting the indices of a face with those of the zone in which it lies. Thus E , the face of the dirhombohedral form corresponding to P , lies in the zones $[OP]$ and $[b, P_{11}]$, whence its indices can be at once obtained, and all the geometrical relations connecting it with P can be proved. Professor Maskelyne has, I believe, already given this method of deducing the indices of the dirhombohedral form in his lectures at Oxford.

XXXII. *On Mr. G. F. Fitzgerald's Paper "On the Mechanical Theory of Crookes's Force."* By OSBORNE REYNOLDS, F.R.S.*

MR. FITZGERALD appears to have overlooked the fact that my paper in the Proceedings of the Royal Society, 1874, "On the Surface Forces caused by Evaporation and Condensation," was published more than a year before Mr. Crookes published any account of the radiometer; otherwise he certainly would not have fallen into the error of supposing that I had concluded that the motion of the arms of the radiometer was mainly due to evaporation and condensation. That such actions cannot explain continuous motion is at once obvious. But then Mr. Fitzgerald fails to notice that on the first page of my paper an experiment is described which proves this very point; and he also fails to notice that all the phenomena I have considered in any way due to evaporation and condensation were essentially intermittent.

It would appear that Mr. Fitzgerald has not read my paper; for after stating that the method by which I "tried to show that a surface, when communicating heat to gas, is subject to an increased pressure is open to the overwhelming objection that this increased pressure would be almost instantaneously transmitted to all parts of the enclosed gas," he devotes some fourteen pages to the attempt to prove the very same thing.

To point out these errors in Mr. Fitzgerald's statements constituted my main object in writing this note; but I would say a few words on the subject in question and Mr. Fitzgerald's treatment of it.

Mr. Fitzgerald bases his theory on Mr. Stoney's view that the phenomena of the radiometer are to be explained by the fundamental assumption "that when two surfaces at different temperatures are in presence of one another with a gas between them, there exists a force tending to separate them." Assuming that it is here meant that the gas should surround the surfaces and not merely exist between them, it may appear at first sight as though this assumption would explain the phenomena; but on closer examination it will appear, as I have previously pointed out, that this is not the case. Under such conditions as are assumed the experiments show that the force would not tend to separate the surfaces, but such forces as there might be would impel both surfaces in the same direction—showing that the force does not act between the two surfaces, but between each surface and the gas which surrounds

* Communicated by the Author.

it—and that the force does not arise from the difference in temperature in the two opposite plates, but from the difference in temperature of the two surfaces of the same plate.

It appears, then, that such a separating force as that assumed by Mr. Fitzgerald would not explain the phenomena in question, and therefore that these phenomena afford us no ground for assuming the existence of such a force. Any reason there may be for assuming such a force must therefore come out of some hypothesis as to the constitution of gas; and in this respect the result of Mr. Fitzgerald's investigation is not very conclusive.

Adopting the hypothesis of Clausius, Mr. Fitzgerald's reasoning leads him to the conclusion that there is no such separating force (see the bottom of page 22). Instead, however, of accepting the conclusion, Mr. Fitzgerald concludes that Clausius is wrong:—"It seems certain that the hypothetical distribution Clausius assumed is not at all adequate to represent the actual one." He then proceeds to modify the expression derived from Clausius' hypothesis so as to make it yield the force for which he is looking; but he attempts no explanation or examination of the physical meaning of such a modification; so that admitting, as I have pointed out, that we have no experimental evidence of such a separating force, Mr. Fitzgerald's investigation clearly affords us none, but, on the other hand, shows either that Clausius is wrong or that there is no such force.

Had Mr. Fitzgerald been true to his mathematics, had he accepted the conclusion that there is no such separating force as he has assumed, and then examined the physical meaning of the modifications of the expressions which he has introduced, I venture to say that he would have found that his modifications of Clausius' hypothesis, on the assumption that the direction of flow of heat is everywhere the same, would correspond with the true expressions of Clausius' hypothesis when there is divergence in the directions along which heat is flowing.

This divergence turns out to be an essential condition in order that there may be force such as that which causes motion in the arms of the radiometer. And since the pressure in the direction of flow is greater or less than the mean pressure, according as the lines of flow diverge or converge, the pressure will be greater against the hot side of a plate and less against the cold side.

When I first suggested that there would be an inequality of gaseous pressure arising from a communication of heat, I had not realized that, besides depending on the quantity of

heat communicated, the inequality would depend on the divergence of the lines of flow—although my explanation, so far as it went, was quite consistent with such a necessity.

The importance of this divergence became clearer to me in November 1877; and I had no sooner recognized it than I perceived a general connexion between the phenomena in question and other phenomena of gases, particularly that of transpiration, or diffusion through porous plugs. I did not then publish any account of the theoretical investigation, because the theory indicated the existence of other phenomena beyond those already known, and I wished to verify these indications of the theory by experiment.

These experiments have occupied considerable time; but they are now completed, and their result is (1) to verify the theoretical revelations as to the existence of a class of very marked and important phenomena which were, so far as I know, previously unsuspected, (2) to establish certain general laws which apply equally to the phenomena of transpiration and those of the radiometer, and (3) to afford an absolute proof that gas possesses dimensional structure (*i. e.* that it is not a continuous plenum), the results of the experiments agreeing with those deduced from the theory in the most definite manner. A paper containing an account of both the theoretical and experimental investigations has been forwarded to the Royal Society.

January 28, 1879.

XXXIII. *The Theory of Binaural Audition. A Contribution to the Theory of Sound.* By ANTON STEINHAUSER*.

[Plate IX.]

INTRODUCTION.

THE theory of Audition may be divided into two portions—that of Monaural Audition, or of hearing with one ear, and that of Binaural Audition, or of hearing with both ears. The former, already treated of in every textbook of Physics, is concerned with explaining the arrangement of the human ear, the function of its separate parts, and, lastly, how the ear is instrumental in the faculty of hearing. The second branch of the subject, which has never, to my knowledge, been yet developed †, has to discuss the general question of hearing,

* Translated and communicated by Prof. Silvanus P. Thompson.

† [For the literature of the subject see:—

Luca, A.—Virchow, *Archiv*, xxv. 1862: “Zur Physiologie und Pathologie des Gehörorganes.”

Rayleigh, Lord.—Proc. Mus. Assoc. 1875–76: “On our Perception of the Direction of a Source of Sound.”

Thompson, S. P.—Rep. Brit. Assoc. 1877: “On Binaural Audition, *Phil. Mag.* S. 5. Vol. 7. No. 42. March 1879.

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with respect in particular to the circumstance that it is performed with two ears. It is concerned, further, in deciding what part binaural hearing plays in the various phenomena of hearing in general, and the various advantages thereby gained.

THEORY OF BINAURAL AUDITION.

Preliminary Observations.

The sound produced by any source of sound is, as is known, brought to our consciousness in the following manner :—The vibrations originated by the sounding body are taken up by solid or fluid bodies which are adjacent to it, or more commonly are taken up by the surrounding atmospheric air, and are thence propagated until they reach either directly, or by reflexion, the auditory passages of our ears, and thus influence the drumskin, the auditory ossicles, and finally the auditory nerve.

Moreover an essential adjunct for fine hearing is the external flap or pinna, which usually acts as a funnel, to conduct into the ear the vibrations which, in consequence of their direction, reach but cannot enter it. These vibrations, travelling through the air in straight lines, we may for brevity call rays of sound, by analogy with the expressions “ rays of light ” and “ rays of heat.”

According to the nature of the path by which the sound-rays reach the *single* ear, we may distinguish between *direct*, *indirect*, and *mixed* monaural hearing.

In *direct monaural audition* the rays proceeding from the sonorous body reach the ear immediately, or in straight lines, and enter the auditory meatus either directly or after undergoing reflexion on the pinna or flap.

In *indirect monaural audition* the rays proceeding from the source of sound do not reach the ear directly (that is to say, not in straight lines), but after undergoing simple reflexion on the ground or on some other surface, or even occasional multiple reflexion, in which case the path travelled by the rays forms a zigzag line. Such a kind of reflexion is in many cases absolutely necessary if the rays of the sonorous body are to reach the ear at all.

In *mixed monaural audition* the rays proceeding from the source of sound reach the ear partly directly, and therefore in straight lines, partly indirectly, or after previously undergoing reflexion, and consequently in zigzag lines.

part 1. Rep. Brit. Assoc. 1878: “Phenomena of Binaural Audition,”
 part 2. *Trans. Assoc. Française*, 1878: “Sur des Phénomènes de l’Audition Binaurculaire.”—TRANSL.]

It might conceivably appear that *direct* audition could only occur under conditions of a special character, and therefore extremely rarely, since the hearer is almost always surrounded by reflecting surfaces, amongst which, of course, the earth must be reckoned.

Figure 1 exhibits the aspect of the human head from above, f_1 and f_2 being the surfaces of the pinnae. They make with one another an angle 2β . Hence it is easily perceived that according to the position which the source of sound occupies in the horizon of the hearer, the following cases may be distinguished with respect to binaural audition.

1. The source of sound may be situated within the angle DnC . The hearer then hears directly with both ears; and accordingly there exists a *direct binaural audition* extending through the angle DnC , which is equal to 2β .

2. The source of sound may be situated within one of the angles AnD and BnC . The hearer then hears directly with one ear only, whilst the other ear hears only indirectly in almost every case, on account of a scattering of the sound, analogous to the scattering of rays of light. In consequence a *mixed binaural audition* occurs, and extends, irrespective of the region in which both ears hear directly, through the angle $(AnD + BnC)$, which is equal to $(360 - 4\beta)$.

3. The source of sound may be situated within the angle AnB . The hearer in that case evidently hears only indirectly with both ears; accordingly *indirect binaural audition* occurs, and extends through the angle AnB , which is equal to 2β .

The 360 degrees of the entire circle within which the source of sound is situated are therefore disposed as follows:—

2 β degrees in a region of direct binaural audition ;					
360-4 β	”	”	mixed	”	”
and 2 β	”	”	indirect	”	”

giving the necessary sum of 360 degrees.

The angle β differs naturally in different individuals, and has been diminished particularly in the female sex by the effect of the head-dresses worn for a long period. That this operates prejudicially on the hearing, at least in so far as it diminishes the extent of the direct binaural audition, may be inferred from what has already been said. It will be in the sequel more narrowly examined. It is self-evident that the range both of the direct and of the indirect *monaural* audition extends to 180 degrees.

If the front part of the head is to offer to the sound-rays on their way to the ears no obstacle of such a nature as to di-

minish their quantity, it will be necessary that the pinnæ of the ears should form tangent planes to the sides of the head. This is in reality approximately the case.

Further, to fulfil the condition that no sound coming from the front shall be heard indirectly only, it is necessary that the surfaces of the pinnæ of the ears should not, when produced, intersect in front of the apex of the nose, *n* (figure 1) ; for in that case no sound-ray proceeding from the space included between the produced surfaces and the face could enter either ear directly.

Every portion of the pinna which reflects into the auditory meatus a sound-ray that falls upon it may be called, with respect to the direction of this sound-ray, an *effective element* of the pinna. The sum of all the effective elements for sound-rays in any given direction constitutes, for that direction, the *effective surface* of the pinna.

The effective surface of the pinna is consequently different for every different direction of sound-rays. Also the intensity with which the sound is perceived depends upon the magnitude of the effective surface. If then, as is found to be the case, the intensity of the sensation of sound be not essentially, or even disproportionately changed by a turn of the head (which in effect is equivalent to a change in direction of the sound-rays), it follows that *the magnitude of the effective surface must be nearly equal for all possible directions of sound-rays*.

The pinna therefore fulfils a special service as a reflecting curved surface ; and it might be possible, if time allowed of a protracted and attentive study of the point, to discover the particular purpose of each of its portions, and to discover its normal form.

In its essential construction the pinna of the human ear consists of a funnel to collect the sound, and a reflector. The former, *a* (figure 2), serves to take up the sound-rays that come from the side, and behind ; the latter, *b*, to reflect those rays which come from the front, and also from the side, this reflexion being frequently accomplished with the aid of the rim of the ear *S*, as figure 2 also shows.

Before turning to the theory of binaural audition, the question might be discussed why a man possesses two ears. For this fact, the following reasons might be assigned :—

First, the law of symmetry. This reason, however, is open to exception, since a single organ of hearing situated on the axis of symmetry of the body would not destroy its symmetry.

Secondly. The beneficial provision of nature which endows

man with organs in reserve. This ground is also not unassailable, since indeed it is nature which permits mankind to be overtaken by disease which, on account of the close connexion between the double organ, almost invariably attacks both organs. So, usually, blindness attacks both eyes, deafness both ears, excepting always the comparatively few cases in which the imprudence of men, or accident, has destroyed one of the two similar organs.

Thirdly. A certain faculty, perhaps, which man acquires from the possession of two ears, and therefore by hearing with two ears. We arrive at the conjecture that this may be the case by bringing into comparison two analogous facts in acoustics and optics:—that, in many cases at least, the place in which the source of sound is situated may be tolerably correctly judged of from the sensation of the sound; and that man acquires a special faculty by the possession of two eyes, which, as is known, affords him a perception of distance.

Now it will be shown in the sequel, that the power which man acquires in certain cases by hearing with two ears, consists in the faculty of discriminating *the direction* in which the source of sound is situated.

On the other hand, *the distance* of the source of sound can only be estimated approximately from the relative intensity of the sensation of sound, *if the source of sound be known*; for then the distance at which the source of sound is situated can be empirically determined from the difference between the perceived intensity and the known absolute intensity, or that which the source of sound would have in the immediate neighbourhood. In this manner, the distance of an enemy is inferred in warfare from the intensity of the cannonading, or the distance of a carriage from the loudness of its rumbling.

It has been shown that we may distinguish between direct, indirect, and mixed binaural audition; the next branch of our subject is therefore the theory of direct binaural audition.

1. *The Theory of Direct Binaural Audition.*

1. Let AA' be the direction of vision, or line of sight; ab and ac (or f_1 and f_2) the effective surfaces of the two pinnae for the (approximately) parallel rays of sound S , α the angle which these rays make with the line of sight, and β the angle between the surfaces of the pinnae and the line of sight. Then, if de is perpendicular to S , ad ($=m$) measures the number of sound-rays which reach the left ear, and ae the number of sound-rays which reach the right ear.

We have then :—

$$\begin{aligned} m &= f_1 \sin (\alpha + \beta), \\ n &= f_2 \sin (\beta - \alpha); \end{aligned}$$

consequently if, as is previously admitted, $f_1 = f_2 = f$, we have the equation

$$\frac{m}{n} = \frac{\sin (\alpha + \beta)}{\sin (\beta - \alpha)}.$$

Developing the sines

$$\frac{m}{n} = \frac{\sin \alpha \cos \beta + \cos \alpha \sin \beta}{\sin \beta \cos \alpha - \cos \beta \sin \alpha};$$

and dividing numerator and denominator of right-hand member by $\cos \alpha \cos \beta$,

$$\frac{m}{n} = \frac{\tan \alpha + \tan \beta}{\tan \beta - \tan \alpha};$$

from which we may further obtain

$$\frac{m+n}{m-n} = \frac{\tan \beta}{\tan \alpha};$$

whence

$$\tan \alpha = \frac{m-n}{m+n} \tan \beta (1)$$

Further, let i_1 and i_2 be the intensities with which the sound which comes in the direction S is perceived in the left and right ears respectively; then the principal considerations are expressed in the following equations :—

$$\frac{m}{n} = \frac{i_1}{i_2};$$

and

$$\frac{m-n}{m+n} = \frac{i_1 - i_2}{i_1 + i_2}.$$

Substituting this expression in equation (1), we obtain

$$\tan \alpha = \frac{i_1 - i_2}{i_1 + i_2} \tan \beta; (2)$$

from which observe that *the direction in which a source of sound is situated may be estimated by the different intensities with which a sound is perceived in the two ears.*

That which is here deduced from calculation our ears acquire by practice in the course of time. That practice is acquired in the following way :—The first lesson which the ear learns, using the eye continually as its instructor, is to distinguish the different kinds of sources of sound, as, for example, the clattering wagon, the clanking chain, the barking dog, &c., Conversely the ear thereby acquires the power of recognizing the source of sound, from the nature of the sound-wave. That

source, as in the preceding examples of the wagon, the chain, &c., will therefore be looked out for by the eyes *alone* only so long as the power of drawing a judgment as to the direction is lacking to the ear. From the impressions which the sound coming in a direction now known makes upon the ears, the ears learn at last to recognize the direction in which the source of sound is situated, without needing the further assistance of the eyes. Thus every one always looks for the source of sound straight in front in the line of sight whenever the impressions on both ears possess equal intensity. A coincident result may be deduced from equation (2), in which if $i_1 = i_2$ we have $\tan \alpha = 0$, and hence $\alpha = 0$.

If, however, the effective surface of one of the two pinnæ be artificially enlarged by holding the hollow of the hand behind the ear, and the intensity with which that ear perceives the sound be consequently increased, the source of sound, even if it does not change its real position, appears to move back toward that side on which the pinnæ has thus been enlarged.

Suppose, for example, that, where originally $i_1 = i_2$, by the enlargement of the effective surface of the *left* pinnæ i_1 becomes greater than i_2 . Then in equation (2) the values of the (proper) fraction $\frac{i_1 - i_2}{i_1 + i_2}$, and therefore also the value of $\tan \alpha$ and of the angle α , become greater than they were before; and the direction which is conjecturally assigned to the source of sound is moved on toward the left.

Also by plugging one of the two ears more or less thoroughly with cotton-wool, an apparent change of direction in the source of sound can be brought about.

In all these investigations the eyes must be kept closed, in order that the judgment may not prejudice the illusion. A point of importance in complete coincidence with the foregoing observations is the phenomenon that individuals who do not hear equally well with both ears, always look for the source of sound more toward that side on which the ear of better hearing is situated. Thus if the worse-hearing ear is the left one, and it perceives a sound of intensity i_1 with only the m th part of that intensity, then

$$\begin{aligned} \tan \alpha &= \frac{\frac{i_1 - i_2}{m}}{\frac{i_1}{m} + i_2} \cdot \tan \beta \\ &= \frac{i_1 - m i_2}{i_1 + m i_2} \cdot \tan \beta. \end{aligned}$$

Now the fraction $\frac{i_1 - mi_2}{i_1 + mi_2}$ is in all cases less than the fraction $\frac{i_1 - i_2}{i_1 + i_2}$; hence also $\tan \alpha$ and the angle α become less than they would be if both ears heard equally well. Consequently the direction in which the source of sound appears to be moves towards the *right*, or towards that side on which the ear of better hearing is situated. In the case of a complete deafness of one ear $m = \infty$; hence $\tan \alpha = \frac{-i_2}{+i_2} \cdot \tan \beta = \tan \beta$, and $\alpha = -\beta$, where the negative angle is to be reckoned on the right of the line of sight.

2. Let two consonant tones be produced by organ-pipes (in the apparatus to be described below, which I call a homophone, and which is for the ears the analogue of the stereoscope for the eyes), but in such a manner that one tone, having the intensity i_1 , shall affect the left ear only, and the other tone having the intensity i_2 shall affect only the right ear; then, at least when the eyes are closed, the two impressions unite in a single impression which appears to be produced by a source of sound in the direction α as determined by equation (2) :—

$$\tan \alpha = \frac{i_1 - i_2}{i_1 + i_2} \cdot \tan \beta.$$

This apparatus is shown in plan in figure 4; where abc represents a wooden tube of square cross section, closed at a and c , and provided at b with a mouthpiece through which a continuous blast of air from a bellows may be introduced. Near the ends a and c , two flute organ-pipes, p_1 and p_2 , are inserted into two holes in the tube. The pitch of these pipes is alike; but the relative intensities of their tones can be brought to any desired proportion by means of the valves m and n , which regulate the current of air. The cardboard funnels T_1 and T_2 respectively lead the tones to the ears O_1 and O_2 of the observer whose head is represented at K .

3. Suppose two persons, A and B, for whom the values of β differ, to make an estimation of the direction of a sound under precisely similar conditions, and that in both cases, therefore, the sound-rays and the line of sight include the angle α ; agreeably with that which has preceded, the measurements for reckoning the sound-rays which respectively enter the left and right ears for the two observers will be

and	A. $m_1 = f \sin (\alpha + \beta_1)$ $n_1 = f \sin (\beta_1 - \alpha);$
and	B. $m_2 = f \sin (\alpha + \beta_2)$ $n_2 = f \sin (\beta_2 - \alpha).$

Now, in order to learn whether in the case of a single sensation of sound *the ratio between the intensities of the sensations in the two ears* is the same for both individuals, we have obviously only to determine whether

$$\frac{m_1}{n_1} = \frac{m_2}{n_2},$$

or whether

$$\frac{\sin(\alpha + \beta_1)}{\sin(\beta_1 - \alpha)} = \frac{\sin(\alpha + \beta_2)}{\sin(\beta_2 - \alpha)},$$

since, according to what has already preceded, those intensities are in the proportion of each m to its respective n .

Developing the sines, we get

$$\frac{\sin \alpha \cos \beta_1 + \cos \alpha \sin \beta_1}{\sin \beta_1 \cos \alpha - \cos \beta_1 \sin \alpha} = \frac{\sin \alpha \cos \beta_2 + \cos \alpha \sin \beta_2}{\sin \beta_2 \cos \alpha - \cos \beta_2 \sin \alpha}.$$

Hence, by algebraic proportion,

$$\frac{2 \cos \alpha \sin \beta_1}{2 \sin \alpha \cos \beta_1} = \frac{2 \cos \alpha \sin \beta_2}{2 \sin \alpha \cos \beta_2},$$

$$\frac{\sin \beta_1}{\cos \beta_1} = \frac{\sin \beta_2}{\cos \beta_2},$$

whence

$$\tan \beta_1 = \tan \beta_2,$$

which can only be true in the case proposed if $\beta_1 = \beta_2$.

The premised condition, that the ratio between the intensities with which the two ears of an observer perceive a sound is the same for *two* observers for whom the angle β differs while other conditions remain the same, is therefore inexact; whence *the standard of each individual for the perception of the direction of sounds is dependent upon the angle β included between the effective surfaces of the pinnae of his ears.* Suppose, for example, that in the observer A, $\beta_1 = 25^\circ$, and in the observer B, $\beta_2 = 30^\circ$, and that the angle α which the sound-rays make with the line of sight of each of the observers is 30° ; then:—

For observer A:

$$m_1 = f \sin(\alpha + \beta_1) = f \sin 45^\circ,$$

$$n_1 = f \sin(\beta_1 - \alpha) = f \sin 5^\circ;$$

consequently the ratio of the intensities is

$$m_1 : n_1 = \sin 45^\circ : \sin 5^\circ = 0.707 : 0.087 = 8.1 : 1.$$

For observer B :

$$m_2 = f \sin (\alpha + \beta_2) = f \sin 50^\circ,$$

$$n_2 = f \sin (\beta_2 - \alpha) = f \sin 10^\circ;$$

consequently the ratio of the intensities is

$$m_2 : n_2 = \sin 50^\circ : \sin 10^\circ = 0.766 : 0.173 = 4.4 : 1.$$

The source of sound will therefore be sought for by observer A in a direction 20 degrees to the left of the line of sight when the intensity of the sensation of sound in the left ear is more than eight times as great as that of the sensation in the right ear, but by the observer B when the intensity of the sensation in the left ear is not quite $4\frac{1}{4}$ times as great as that in the right ear.

I think, then, I may not inaccurately conclude that the perception of the direction becomes the more certain as the difference between the intensities of the sensations of sound in the left and right ears is the greater—just as analogously the distance of a body may be the more surely estimated in binocular vision the smaller that distance is, or as the directions of the two optic axes differ more and more widely.

The difference between the two intensities will, under similar conditions, be greatest for that individual for whom the difference $m - n$ has the greatest value. And since

$$\begin{aligned} m - n &= f \sin (\alpha + \beta) - f \sin (\beta - \alpha) \\ &= f (\sin \alpha \cos \beta + \cos \alpha \sin \beta - \sin \beta \cos \alpha + \cos \beta \sin \alpha) \\ &= 2f \sin \alpha \cos \beta, \end{aligned}$$

this value becomes greater as $\cos \beta$ becomes greater, or as angle β itself becomes less; consequently *the smaller the angle included between the line of sight and the surfaces of the pinnae, the more certain will be the perception of the direction of sounds.*

4. We are now prepared to pursue the investigation of the *direction of best hearing*, i. e. of the direction in which a source of sound at a given distance must be situated in order that it may be heard or perceived best.

Hearing is obviously better in proportion as the rays of sound which reach the ears from the source of sound at the given distance are more numerous, or as the dimensions m and n of figure 3 are the greater. Hearing *with the two ears* will therefore be best produced when the value of the sum $m + n$ is greatest. Now it was shown that

$$m = f \sin (\alpha + \beta) = f (\sin \alpha \cos \beta + \cos \alpha \sin \beta),$$

$$n = f \sin (\beta - \alpha) = f (\sin \beta \cos \alpha - \cos \beta \sin \alpha);$$

therefore $(m+n)$ will be a maximum when

$$f(\sin \alpha \cos \beta + \cos \alpha \sin \beta + \sin \beta \cos \alpha - \cos \beta \sin \alpha),$$

or

$$2f \sin \beta \cos \alpha$$

is a maximum. But in this expression, so long as one person only is concerned, the only variable is α ; consequently $2f \sin \beta \cos \alpha$ will have its maximum value, namely $2f \sin \beta$, when $\cos \alpha$ is a maximum, which occurs when $\alpha = 0^\circ$.

We hear best therefore with the two ears when the sound reaches us from the front in the line of sight.

It is in consequence of this that we always turn with our face to the speaker, provided that we possess equally good hearing of the two ears.

If this be not the case, and we hear worse (for example) with the right ear than with the left, and to such an extent that we perceive a sound of intensity i_2 with only the x th part of that intensity, then to find the direction of best binaural audition the problem will be the same as if the right ear were considered to receive only $\frac{n}{x}$ rays of sound instead of n rays, or as if the effective surface of that ear were not f but only $\frac{f}{x}$. Proceeding on this assumption, we see at once that if, in figure 5, $ab=f$, and $ac=\frac{f}{x}$, the straight line de drawn at right angles to the direction of the rays of sound S measures the sum of the rays of sound which reach both ears, and that this straight line is longest when parallel to bc . The best binaural hearing therefore occurs when the direction of the rays of sound is perpendicular to the straight line bc . In order to find the angle α , which gives the direction of best hearing for the case under discussion, we have only to consider that, in figure 5, $be=cd$, also that

$$be=f \cos (\alpha+\beta)$$

or

$$=f \cos \alpha \cos \beta - f \sin \alpha \cos \beta,$$

and that

$$cd=\frac{f}{x}(\cos (\beta-\alpha))$$

or

$$=\frac{f}{x} \cos \beta \cos \alpha + \frac{f}{x} \sin \beta \sin \alpha ;$$

whence

$$f \cos \alpha \cos \beta - f \sin \alpha \cos \beta = \frac{f}{x} \cos \beta \cos \alpha + \frac{f}{x} \sin \beta \sin \alpha .$$

From which it follows that

$$x \cos \alpha \cos \beta - x \sin \alpha \sin \beta = \cos \beta \cos \alpha + \sin \beta \sin \alpha,$$

and, further, that

$$(x-1) \cos \alpha \cos \beta = (x+1) \sin \alpha \sin \beta,$$

whence by algebraic transformation,

$$\tan \alpha = \frac{x-1}{x+1} \cot \beta.$$

Tan α and also angle α increase in value as the proper fraction $\frac{x-1}{x+1}$ increases; but this fraction becomes greater as x increases in value, and x increases as the degree of deafness of one ear increases. It follows, therefore, that the person hard of hearing must, in order to hear best, turn to the speaker his better-hearing ear to a larger degree in proportion as the hearing of the other ear is relatively worse.

In the case of complete deafness of one ear, $x = \infty$, and our formula becomes

$$\tan \alpha = \frac{1 - \frac{1}{x}}{1 - \frac{1}{x}} \cot \beta,$$

or

$$\tan \alpha = \cot \beta,$$

whence

$$\alpha = 90 - \beta,$$

which is the case when the rays of sound are perpendicular to the surface of the hearing ear, and that ear is turned straight towards the speaker.

In the case of two different individuals the angle β is generally different. Suppose two individuals hard of hearing in one ear to an equal degree. Since $\tan \alpha$ and angle α are greater in proportion as $\cot \beta$ is greater and as β is less, it follows that *persons equally hard of hearing in one ear* must, in order to hear as well as possible, turn the better-hearing ear the *more* towards the speaker in proportion as the angle which the surfaces of their pinnæ make with the line of sight is *less*. The cases of hardness of hearing *in both ears* may be separated into two: (1) the cases in which the two ears are equally hard of hearing; (2) the cases in which they are unequally hard of hearing.

In the *first case* suppose, for example, that the left ear

hears only with intensity $\frac{i_1}{x}$ instead of intensity i_1 , and the right ear with intensity $\frac{i_2}{x}$ instead of intensity i_2 . The direction in which the source of sound is estimated to be situated is, in accordance with our preceding paragraphs, given by the equation

$$\tan \alpha = \frac{\frac{i_1}{x} - \frac{i_2}{x}}{\frac{i_1}{x} + \frac{i_2}{x}},$$

which cancels into

$$\tan \alpha = \frac{i_1 - i_2}{i_1 + i_2} \tan \beta.$$

But since this equation gives the same angle which was found when both ears possessed normal hearing powers, it follows that *the power of perception of the direction of a sound is not vitiated by an equal hardness of hearing of both ears*. This is indeed reasonable on other grounds, since the distance of the source of sound on which the intensities of the two sensations is dependent, but of which the ratio between those intensities is independent, exercises no influence on the perception of the direction.

The *second case* may obviously be referred to cases already treated in detail, if we make the supposition that we may take the "effective surfaces" of magnitudes proportional to the respective powers of the two ears.

Best hearing with one ear must obviously occur when $m = f \sin(\alpha + \beta)$ becomes a maximum, which, since β remains constant for a single individual, will occur when $\sin(\alpha + \beta) = 1$, or when $(\alpha + \beta) = 90^\circ$, or, finally, when $\alpha = 90^\circ - \beta$. This agrees with figure 3, if the direction of the rays of sound be made perpendicular to the surface of the pinna f_1 , in which case $m = f_1 = f$.

Finally, let us consider that the greatest number of rays of sound which can be received by *both* ears is measured, according to what has been established early in § 4, by the expression $2f \sin \beta$, in which case the source of sound is situated in the line of sight; further, that the greatest number of rays of sound which can be received by *one* ear is measured by f , in which case the ear in question is turned towards the source of sound. Hence it follows that, assuming the specified positions of best hearing, the hearer will hear better with one ear than with both ears, if in his case

$$f > 2f \sin \beta.$$

But if this is so,

$$1 > 2 \sin \beta,$$

or

$$\frac{1}{2} > \sin \beta;$$

and in this case,

$$\beta < 30^\circ.$$

Now, since it is the fact that for most persons $\beta < 30^\circ$, *this explains the well-known position assumed by the listener who turns one ear towards the source of sound.*

5. In the theory of Binaural Audition developed in the preceding paragraphs, it has been tacitly assumed throughout that the source of sound should be situated in that plane (usually horizontal) in which the line of sight and the line joining the middle points of the two pinnæ are situated. This is not, however, always the case; for while the line of sight may be horizontal, the source of sound may be situated above or below the imaginary horizontal plane. Hence it is necessary to enlarge the theory of binaural audition in this respect. In figure 6, which is drawn in so-called parallel perspective, let AA' be the horizontal line of sight; then f_2 or $adgc$, and f_1 or $adeb$ are the effective surfaces of the pinnæ, each enclosing the angle β between it and the line of sight. Let the plane dk be a prolongation of one of these planes, f_2 . Lastly, let aM be the direction of any ray of sound (the position being a general one) whose projections are $M''a$ upon the vertical plane dk , and $M'a$ upon the horizontal plane aq . Then to reckon the rays of sound (assumed parallel to one another) which meet the surfaces f_1 and f_2 in the given direction defined now by the angle γ vertically, and by the angle α horizontally, we must take as measures of the number of those rays the cross sections which can be led orthogonally through the two quadrangular prisms which have respectively for their bases the surfaces f_1 and f_2 , and whose long edges are parallel to the direction of the rays of sound aM .

These cross sections, which may be drawn upon one common plane perpendicular to the direction of the rays of sound, may be regarded as the projections of the surfaces f_1 and f_2 upon that plane. And since the area of the projection of a plane figure is equal to the product of the area of that figure into the cosine of the angle included between the plane of the figure and the plane of projection, we shall be able to find the areas of the cross sections, provided we first know the angles w_1 and w_2 which the direction of the rays of sound makes with the surfaces f_1 and f_2 respectively. Now the plane taken normally to the direction of the rays of sound is the plane in which the

cross sections are situated. Let us call the angles included between that plane and the surfaces f_1 and f_2 respectively ϕ_1 and ϕ_2 . We know also that $90^\circ - w_1 = \phi_1$, and $90^\circ - w_2 = \phi_2$; and hence the areas of the cross sections f'_1 and f'_2 are respectively $f_1 \cos \phi_1$, and $f_2 \cos \phi_2$.

The remainder of the calculation follows the course thus indicated. From figure 6 we learn that

$$MM' = M'n = aM \sin w_2,$$

and
$$\sin w_2 = \frac{M'n}{aM}.$$

Further, since angle $A'an = \beta$,

$$M'n = aM \sin (\beta - \alpha).$$

But

$$aM = \frac{aM'}{\cos \gamma},$$

whence

$$\sin w_2 = \frac{aM' \sin (\beta - \alpha)}{\frac{aM'}{\cos \gamma}},$$

and

$$\sin w_2 = \sin (\beta - \alpha) \cos \gamma.$$

Here w_2 is the angle included between the direction of the rays of sound and the surface f_2 , since that angle is, as we know, also the angle which that direction includes with the projection of the surface f_2 .

Now imagine the surface f_2 rotated about the straight line ad to the right until the line ac forms a continuation of ba . The angle β which this surface included with the line of sight is thereby changed into the angle $(180 - \beta)$, while the surface f_2 has changed into the position of the surface f_1 . Hence, in order to find the angle w_1 , which is included between the direction of the rays of sound and the surface f_1 , we have merely to replace β in the preceding formulæ by the value $(180^\circ - \beta)$.

We have therefore

$$\begin{aligned} \sin w_1 &= \sin [180^\circ - \beta - \alpha] \cos \gamma, \\ &= \sin (\alpha + \beta) \cos \gamma. \end{aligned}$$

Let ϕ_1 and ϕ_2 be the angles which are respectively included between the surfaces f_1 and f_2 and the plane which we have taken normally to the direction of the rays of sound; then,

since $\phi_1 = 90^\circ - w_1$, and $\phi_2 = 90^\circ - w_2$, we have

$$\begin{aligned}\cos \phi_1 &= \sin w_1 = \sin (\alpha + \beta) \cos \gamma, \\ \cos \phi_2 &= \sin w_2 = \sin (\beta - \alpha) \cos \gamma;\end{aligned}$$

and the areas of the cross sections of the parallel pencils of rays of sound which reach the left and right pinnæ, respectively, are

$$\begin{aligned}f'_1 &= f_1 \cos \phi_1 = f_1 \sin (\alpha + \beta) \cos \gamma, \\ f'_2 &= f_2 \cos \phi_2 = f_2 \sin (\beta - \alpha) \cos \gamma.\end{aligned}$$

But as these cross sections constitute, as has already been shown, the measures by which the rays of sound which respectively reach the left and right pinnæ are to be reckoned, we may draw the following conclusions:—

(1) That, with respect to the angle of altitude γ , the smaller this angle becomes the better will the hearing be, since in the sum

$$f'_1 + f'_2 = f_1 \sin (\alpha + \beta) \cos \gamma + f_2 \sin (\beta - \alpha) \cos \gamma,$$

which must be a maximum for the condition of best hearing, both terms have maximum value when $\gamma = 0$.

We hear therefore binaurally the best, relatively, when the source of sound is situated in that plane in which are situated the line of sight and the line joining the middle points of the pinnæ ("plane of best hearing"), and the best, absolutely, when it is situated in the line of sight.

(2) That the intensities i_1 and i_2 with which a sound is perceived in the two ears, for any values of γ whatever, always remain equal so long as $\alpha = 0$, since then

$$f'_1 = f_1 \sin \beta \cos \gamma = f_2 \sin \beta \cos \gamma,$$

and, because $f_1 = f_2 = f$,

$$f'_1 = f \sin \beta \cos \gamma, \text{ and } f'_2 = f \sin \beta \cos \gamma.$$

(3) That the intensities i'_1 and i'_2 , with which a sound coming from *above* or *below* (in the latter case γ is negative) is perceived in the two ears, are in the ratio $f'_1 : f'_2$, or as

$$\sin (\alpha + \beta) : \sin (\beta - \alpha).$$

And since also in this case the proportion

$$i_1 : i_2 = \sin (\alpha + \beta) : \sin (\beta - \alpha)$$

holds good, which was deduced for the case in which the source of sound was situated in the plane of best hearing, it follows, either by calculation from equation (2), that

$$\tan \alpha = \frac{i_1 - i_2}{i_1 + i_2} \tan \beta,$$

or by what we know from practical experience, *that in the case of hearing a sound coming from above or below, we are able to estimate, from the relative intensities with which the sound is perceived in the two ears, the azimuth of the rays of sound as projected upon the plane of best hearing.*

This explains the method which we pursue to discover the position of a source of sound whose situation is unknown, and which consists in a motion of the head.

For example, to find a source of sound situated anywhere *above*, we have only to turn the head about a vertical axis until we hear equally with both ears and with the greatest total intensity. Then the line of sight coincides with the horizontal projection of the direction of the sound. The head must then be turned upwards about a horizontal axis at right angles to the line of sight, as long as the intensity of the sensation increases.

When this reaches its maximum, and is equally great for both ears, then the source of sound must be situated in the line of sight.

And here we must remark on the essential importance of the actual conditions—that the ears and eyes, being alike attached to the head, share its movements, and that they are situated at almost the same height above the ground.

Should we perceive a falling-off in the intensity of the sound on raising the head, this would be an indication that the source of sound is situated *below* the plane of best hearing, and that we should be able by sinking the head to bring the source of sound into the line of sight in the manner just described for a sound above the head.

It is intelligible and natural that, where (as in an instrument for measuring altitude and azimuth) we have two separate motions of rotation at right angles to one another, it is immaterial whether the movements in the two directions be executed separately or by a simultaneous motion of the head.

This occurs, for example, when we try to find a lark which we may hear singing above a field. We raise the head, making an arbitrary guess at the position of the lark in the sky. Then we turn the head about, led meanwhile *by ear* until we hear equally well with the two ears and with the greatest possible intensity; and simultaneously we perceive the lark in the line of sight. We do not, therefore, as it might be conjectured in this case, seek for the source of sound by means of the eyes, but *by means of the ears.*

[To be continued.]

XXXIV. *On the Determination of the Variation of the Thermal Conductivity of Metals with Temperature, by means of the permanent Curve of Temperature along a uniform thin Rod heated at one end.* By OLIVER J. LODGE, D.Sc., Lecturer on Applied Mathematics and Mechanics at University College, London*.

[Plate X.]

1. **T**HE approximate theory of the flow of heat down a uniform rod heated at one end and exposed to cooling influences everywhere else was, I believe, given by Biot before the time of Fourier, and was also verified experimentally by him. The "constants" occurring in Biot's equation to the curve of temperature have long been known to be variable; and numerous experiments have been made to determine empirically in what manner they depend upon the temperature. Nevertheless Biot's method in its original form has been frequently employed by subsequent observers in order to compare the conductivities of different metals—by Ingenhousz for instance, by Despretz, and even in the far more accurate experiments of Professors Wiedemann and Franz. Principal Forbes, on the other hand, devised and executed a method which was quite independent of any equations to curves except those deduced from experiment; and by graphical and other laborious methods he determined both the absolute conductivity of wrought iron and its variation with temperature. But, as far as I know, no recalculation of the curve of temperature with the improved data now accessible has been made. It therefore seemed worth while to obtain as close an approximation to the equation of the true curve of temperature as is practicable without cumbrous integration, and to see how far the improvement affects the results of those experimenters who, unlike Forbes, depended on the theoretical curve of temperature. Moreover it seemed probable that the more accurate equation to the curve would enable the variation in conductivity of the rod with temperature to be calculated in some moderately simple manner, and with far less labour than that gone through by Forbes. Forbes's methods are perfect; the only objection to them is the excessive tediousness of the process of discussing the experimental results. And as it is a most important research at the present time to compare exactly the law of variation of thermal and electrical conductivity in the same piece of material, it seemed desirable to have some means of calculating the law of thermal variation from some simple

* Communicated by the Physical Society, an abstract having been read on the 8th of February, 1879.

experimental data; and the long-thin-rod form of experiment is evidently suitable for observing the variation of *electric* conductivity with temperature.

The following paper is unfortunately rather long; but the length is due to the necessity of fully discussing experimental results, and I have skipped nearly all the mathematical steps, as they are elementary and of no interest.

2. Consider a thin uniform infinitely long rod, of perimeter p and cross section q , made of a material whose specific conductivity is k , density ρ , and specific heat c . Let this rod be surrounded by an enclosure at the absolute temperature v_0 , and let one point of the rod (which we will call the origin) be kept by some means at a constant temperature Θ above that of the enclosure; then heat will flow from this point along the rod and will be dissipated at its surface, and the temperature of every point of the rod will rise at a rate proportional to the excess of the quantity of heat which it gains per second by conduction, over that which is dissipated by radiation and convection. After a long time, however, this excess of heat vanishes, and the temperature of any point of the rod ceases to rise, having attained a constant temperature θ above that of the enclosure—its absolute temperature, t , being therefore $\theta + v_0$. (I will adhere to the letter v for temperatures reckoned in Centigrade degrees from absolute zero, and to θ for temperatures reckoned from the temperature of the enclosure as zero. We shall have to use occasionally the Centigrade zero—the temperature of melting ice; and temperatures reckoned from it may be denoted by t .)

The heat which flows in unit time past any cross section of the rod at a distance x from the origin will be

$$-kq \frac{d\theta}{dx};$$

and the gain of heat per second by an element of volume qdx in this position will be the differential of this quantity, or

$$kq d \frac{d\theta}{dx}.$$

If every unit area of the surface of the rod at this point is losing by radiation and convection the quantity H per second, the rate of loss of heat by the surface of the element is

$$H p dx,$$

the product pdx being the area of its surface. As long as the temperature of the element is rising, the rate of rise of temperature will be the difference of the last two expressions divided by the thermal capacity of the element—that is, divided by

$cpqdx$; but when the permanent state is reached, the heat gained and the heat lost become equal, and their equality is the fundamental differential equation for the permanent state of a rod, viz.

$$kqd\frac{d\theta}{dx} = Hpdx,$$

or

$$\frac{d^2\theta}{dx^2} = \frac{Hp}{kq}. \quad (1)$$

The four quantities which enter into the right-hand side of this equation are all variables, and may be expressed as functions of θ . It has, however, been always assumed, in the approximate theory hitherto used, that H is the only variable, and that it is simply proportional to the excess of temperature, and can be written

$$H = h\theta,$$

where h is a constant. (This is called Newton's law.) What we now want to do, however, is to take into account the variability of all these constants as far as present experimental results will enable us to do so, and then to integrate the above equation to as great a degree of accuracy as is easily possible.

3. Now, if an isolated body of volume V , surface S , density D , and specific heat C loses from each unit of surface a quantity of heat H per second, then its rate of fall of temperature is

$$-\frac{dv}{d\tau} \text{ or } \dot{v} = \frac{SH}{VDC};$$

writing \dot{v} or $\dot{\theta}$ for the essentially positive quantity $-\frac{dv}{d\tau}$. Hence an element of the rod (§ 2), if isolated from its neighbours by two flat impervious films, will cool at the rate

$$\dot{\theta} = \frac{Hpdx}{cpdx},$$

whence its rate of loss of heat per unit of surface is

$$H = \frac{qcp}{p}\dot{\theta}. \quad (2)$$

Substituting this value of H in equation (1), it becomes

$$\frac{d^2\theta}{dx^2} = \frac{cp}{k}\dot{\theta}, \quad (3)$$

which is precisely the *form* of the equation to the variable flow of heat through a slab*, though $\dot{\theta}$ has there a very diffe-

* See Everett, Trans. Roy. Soc. Edinb. vol. xxii.

rent meaning. The product $c\rho$ is the capacity for heat of unit volume of the rod (ρ being the mass of unit volume); hence $\frac{k}{c\rho}$ is the conductivity in terms of a unit of heat which can raise unit volume of the rod one degree. This Professor Maxwell calls the *thermometric* conductivity*, as distinguished from the calorimetric conductivity k .

4. In equation (3), $\dot{\theta}$ is a function of θ ; and if the element were not supplied with heat, it would cool at the rate $\dot{\theta}$, and both θ and $\dot{\theta}$ would be functions of the time. But when heat is supplied to the element at a compensating rate by its neighbours, θ is constant, and therefore also $\dot{\theta}$ is constant as regards time; yet still the rod will emit heat at the same rate H as before, and $\dot{\theta}$ will be the same function of θ as if it were actually cooling: hence $\dot{\theta}$ was called by Forbes the *statical* rate of cooling.

The relation between $\dot{\theta}$ and θ for a cooling body, or the curve which expresses $\dot{\theta}$ as a function of θ , has been investigated experimentally by Dulong and Petit, and found to be of an exponential form. Newton's law made it a straight line. Forbes called it the secondary curve of cooling, and found a point of inflection on it for a long body cooling in air. For a rod in a permanent state, θ is a function of x ; and the curve θ, x is the statical curve of temperature down the rod, and is the one we want to investigate. The curve $\dot{\theta}, x$ is what Forbes called the statical curve of cooling. Finally, the curve expressing θ as a function of time is the ordinary curve of cooling of a body. The general nature of these last three curves is the same, and depends on that of the first curve $\dot{\theta}, \theta$. The first rough approximation to them is that they are all logarithmic, this being a consequence of the hypothesis that the first curve is a straight line. I suppose that the fact that $\dot{\theta}$ is only *apparently* a function of the time renders abortive the analogy between equation (3) and the equation to the variable flow of heat in a slab.

On the Variation of $\frac{k}{c\rho}$ with Temperature as at present known.

5. Professor Tait has given theoretical reasons for assuming the conductivity (*i. e.* the *thermometric* conductivity) of every substance to be inversely proportional to the absolute temperature†; but I do not know whether he lays much stress upon

* See Maxwell's 'Theory of Heat,' p. 235.

See 'Recent Advances,' p. 271.

the correctness of the theory. At any rate it does not seem to agree very well with the results of experiment, except in the case of iron. The experiments of Principal Forbes* established the fact that the thermometric conductivity of wrought iron is nearly inversely proportional to the absolute temperature; but the agreement is not quite perfect, as the following Table shows.

Centigrade temperature, t .	Conductivity at the temp. t , as found by Forbes, reduced to C.G.S. units, $\frac{k}{cp}$.	Product of conductivity and absolute temperature, $(274+t) \frac{k}{cp}$.	Product which is more nearly constant, $(400+t) \frac{k}{cp}$.	Product, $(308+t) \frac{k}{cp}$.
0	·2331	63·87	93·24	72·26
50	·1995	64·65	89·78	71·82
100	·1764	65·98	88·20	72·32
150	·1629	69·08	89·60	74·93
200	·1528	72·41	91·68	
250	·1440	75·43	93·60	

The third column contains the numbers which ought to be constant if the theory were accurate. The fourth column contains numbers calculated on the hypothesis that the conductivity varies inversely as the absolute temperature increased by some constant, say by 126. These numbers agree with one another rather better than those in the preceding column; but still there is a regular divergence perceptible between the hypothesis and the experimental results, especially at high temperatures.

The results at the higher temperatures, however, do not seem to have been regarded by Principal Forbes as equally dependable; for he gives an empirical formula for the conductivity at any Centigrade temperature t which does not agree very closely with the experimental results at high temperatures, saying, "I have assumed that the most trustworthy part of the observational curves are those between the actual temperatures of 40° and 160° , and that within moderate limits the conductivity may be represented in terms of the temperature by such a formula as"

$$\frac{k}{cp} = A + Bt + Ct^2,$$

* Trans. Roy. Soc. Edinb. vols. xxiii. & xxiv.

where the constants for the best of his two bars, when reduced to the C.G.S. system, are

$$A = \cdot 2331, \quad B = -\cdot 00755, \quad C = \cdot 00000189.$$

6. Forbes's formula may therefore be written

$$\left(\frac{k}{c\rho}\right)_{\text{Fe}} = \cdot 2331(1 - \cdot 00324t + \cdot 0000081t^2);$$

and this may be very accurately expressed by a form more suitable for our present purpose, $\frac{A}{b+t}$. For this last may be regarded as the sum of an infinite geometrical progression with ratio $-\frac{t}{b}$, and may be written, without approximation,

$$\frac{A}{b+t} = \frac{A}{b} \left(1 - \frac{t}{b} + \frac{t^2}{b^2} - \dots + \frac{t^n}{(b+t)b^{n-1}}\right),$$

stopping at any term one likes, and multiplying it by $\frac{b}{b+t}$ instead of writing the remaining terms.

Now if T be the highest temperature to which the formula is required to apply, the average temperature $\frac{1}{2}T$ may be introduced into the denominator of the last term instead of the variable t , without making much difference; and the above may be written approximately, stopping at the third term,

$$\frac{A}{b+t} \doteq \frac{A}{b} \left(1 - \frac{t}{b} + \frac{t^2}{b(b + \frac{1}{2}T)}\right)^*.$$

This expression agrees very well with Forbes's formula for a range of 200° ; for taking $b = 308$, $\frac{1}{2}T = 100$, and $\frac{A}{b} = \cdot 2331$, it becomes

$$\frac{A}{308+t} = \cdot 2331(1 - \cdot 00325t + \cdot 000008t^2).$$

Hence I shall assume that the results of Forbes may be summed up in the equation

$$\left(\frac{k}{c\rho}\right)_{\text{Fe}} = \frac{A}{308+t} = \frac{308}{308+t} \cdot \frac{k_0}{c_0\rho_0} \dots \quad (4')$$

The numbers in the last column of the preceding Table give the values of the "constant" A , for the temperatures considered most trustworthy by Forbes.

7. The variations of conductivity with temperature have also been investigated, by a method depending on fluctuating

* The symbol \doteq is used merely to signify approximate equality.

temperature, for copper and iron by Ångström*; and his result for iron, reduced to the C.G.S. scale, can be expressed in the following way,

$$\left(\frac{k}{c\rho}\right)_{\text{Fe}} = .2143(1 - .002874t),$$

which may be considered as equivalent to $\frac{A'}{348+t}$ for a small range of temperature.

The results of Ångström for copper are summed up in the following formula,

$$\left(\frac{k}{c\rho}\right)_{\text{Cu}} = 1.163(1 - .001519t),$$

which may be expressed as $\frac{B}{658+t}$ when t is small.

8. The law of variation of the ordinary or calorimetric conductivity k can, of course, be deduced from the above by multiplying them by the value of the product $c\rho$, with each of its factors expressed as a function of the temperature—the one factor from the experiments of Bède on specific heat, the other from the expansion experiments of Fizeau. This Professor W. Dumas† has done. Using a mean coefficient of expansion between 0° and 100° C., he writes for the density of iron,

$$\rho = 7.7799(1 - .00003684t);$$

and for its specific heat,

$$c = .1053(1 + .001348t).$$

Multiplying Ångström's value of $\frac{k}{c\rho}$ for iron by the product of these two quantities, we obtain

$$k = .1862(1 - .00156t).$$

And multiplying Forbes's numbers for $\frac{k}{c\rho}$ (nominally for k), as reduced to the C.G.S. system by Dr. Everett, on page 44 of his book on Units (first edition), by the variation factor $(1 + .001311t)$, we obtain the real values of k at the different Centigrade temperatures t according to Forbes's experiments,

Phil. Mag. vol. xxv. 1863.

† Pogg. Ann. cxxix. See also Wüllner, *Exp. Physik*, vol. iii. pp. 286 & 287.

<i>t.</i>	<i>k.</i>
0	·207
25	·1975
50	·189
75	·182
100	·177
200	·171

numbers which can be expressed with considerable accuracy by the formula

$$k = \cdot 207(1 - \cdot 00144t).$$

9. The result of all this appears, then, to be that, as far as experiment has hitherto gone, the conductivity (both calorimetric and thermometric) of the metals copper and iron may be expressed with moderate correctness as a linear function of the Centigrade temperature, with a negative value for $\frac{dk}{dt}$, or

$$\frac{k}{cp} = A - Bt,$$

and that it may be expressed with a trifle more accuracy by an inverse function of the temperature,

$$\frac{k}{cp} = \frac{A}{b+t},$$

because this may be written very approximately, when *b* is much bigger than *t*,

$$\frac{A}{b} \left(1 - \frac{t}{b} + \frac{t^2}{b^2} \right).$$

I will therefore assume that the variation of conductivity in any metal for moderate ranges of temperature is expressed by the equation

$$k = \frac{Acp}{b+t}, \quad (4)$$

and that, since the variations of density and specific heat are known, the law of variation of conductivity in different metals is sufficiently discovered as soon as we have found the value of the constant *b* for each metal. Our object then is to find a mode of calculating *b*.

On the Variation of θ with Temperature.

10. We have now to consider in what way the other factor of the right-hand side of equation (3), namely θ , may be expressed as a function of the temperature.

The magnificent researches of Dulong and Petit on this

point have established the following expression for the velocity of cooling of a body whose absolute temperature is v , in an enclosure of absolute temperature v_0 , containing gas at the pressure ϖ ,

$$\dot{v} = P(a^v - a^{v_0}) + Q\varpi^g(v - v_0)^{1.232},$$

which may also be written in terms of the excess of temperature $\theta = v - v_0$, thus,

$$\dot{\theta} = Pa^{v_0}(a^\theta - 1) + Q\varpi^g\theta^{1.232}. \quad . \quad . \quad . \quad (5)$$

The first term is the rate of cooling by *radiation*; the second term is the rate of cooling by *convection*. In other words, $Q=0$ in a vacuum; and P is small if the surface of the body is silvered, but great if it be lampblackened. The constant g depends on the nature of the gas; for air it is .45; but a is said to be a universal constant, and equal to 1.0077. Although this is an empirical formula, it is perhaps the most perfect example of such a formula that we have, and it expresses Dulong's results thoroughly well. The necessity of such an elaborate expression has been called in question by Narr; but his experiments, so far as they go, seem rather to confirm than to upset this expression; and it has been in the main verified by Provostaye and Desains.

11. Some caution, however, seems advisable with respect to the second term, which expresses the loss by convection as constant power of the excess of temperature; because it was found by Principal Forbes that when the excess of temperature was very small, the loss by convection was almost inappreciable; and he suggested the viscosity of the air to account for this—some finite excess of temperature being required to set convection-currents going. The point of inflection, moreover, which Forbes found on the curve $\dot{\theta}$, θ at a high temperature (see § 4) is wholly unaccounted for by Dulong's expression; but it is probable that here the experiments of Dulong are the most accurate, and that the contrary flexure of Forbes's curve was due to waves of heat in the elongated mass whose cooling he investigated*. Experiments on the rate of cooling of bodies at the ordinary pressure of the atmosphere have been made by Mr. Macfarlane and by Mr. Nichol; but their excesses of temperature only went as high as 60° (see Everett, 'On C.G.S. Units,' p. 50). I believe Professors Ayrton and Perry have some results not yet published.

On another ground also caution seems to be rendered necessary by the kinetic theory of gases, as illustrated in the

* Or see Professor Tait's explanation given to the Royal Society of Edinburgh on the 20th of last January ('Nature,' No. 486, p. 379).

experimental investigations of Mr. Crookes; for if the enclosure containing the cooling body be gradually exhausted of air, so that π progressively diminishes, a discontinuity, in the direction of a sudden increase in the rate of cooling, would probably arise at the instant when the average free path of the molecules was long enough to reach from the surface of the cooling body to that of the enclosure. And it is probable that for exhaustions higher than this the law of cooling is different, and in all probability simpler than it was when the heat had to be conveyed between the surfaces by the unsystematic and irregular agency of convection-currents, a process of true gaseous conduction then setting in. This is a point which should be attended to in subsequent investigations; and it would be an important though somewhat difficult research to discover experimentally the law of cooling and its alteration with pressure when the distance between the cooling body and enclosure is less than the free path of the molecules: probably it could be more readily deduced from theory. It is not likely, however, that any of the investigators on the law of cooling hitherto have attained an exhaustion any thing like so perfect as this.

12. These objections, however, only apply to the *convection* part of the formula (5); and I will assume that the radiation part

$$\dot{\theta} = Pa^{v_0}(a^\theta - 1) \quad \dots \dots \dots (5')$$

is practically true as it stands. Since this, however, is not a very simple function for a second differential coefficient like (3), it will be well to see with what amount of accuracy we may expand it into a series and neglect higher terms. The expansion is

$$\dot{\theta} = Pa^{v_0} \left(\theta \log a + \frac{1}{2} (\theta \log a)^2 + \frac{1}{6} (\theta \log a)^3 + \frac{1}{24} (\theta \log a)^4 + \dots \right) \quad (6)$$

which may be conveniently written

$$\dot{\theta} = \frac{2Pa^{v_0}}{(\log a)^2} \cdot \theta \left(\frac{2}{\log a} + \theta + \frac{1}{3} \theta^2 \log a + \frac{1}{12} \theta^3 (\log a)^2 + \dots \right),$$

or, putting in the numerical value of a , viz. 1.0077 (that is, putting $\log_e a = .0076$),

$$\dot{\theta} = C\theta(266.6 + \theta + .0025\theta^2 + .000005\theta^3 + \dots),$$

or

$$\dot{\theta} = C\theta \left(267 + \theta + \frac{\theta^2}{400} + \frac{\theta^3}{200,000} + \frac{\theta^4}{18 \times 10^8} + \dots \right).$$

Remember that θ is to be ultimately the excess of the tempe-

perature of any point of the rod over that of the enclosure. It may be any thing between 0° and 150° ; but it is not likely in ordinary experiments to go above 200° . The terms of the above series for the extreme case $\theta = 200$ are

$$267 + 200 + 100 + 40 + 1,$$

where only the last term, containing the fourth power of the temperature, can be regarded as quite negligible. But for the more likely case of $\theta = 100$, the terms of the series are

$$267 + 100 + 25 + 5 + \frac{1}{18},$$

where the term containing the cube of θ is not of much consequence. If, however, it were wished not to go higher than the second power, the term containing the cube need not be neglected, but a mean value of it may be added to the coefficient of the second power of θ . Thus if Θ be the highest temperature taken notice of (*i. e.* the temperature of the origin in the case of the rod, the initial temperature in the case of a cooling body),

$$\dot{\theta} = C\theta \left\{ 267 + \theta + \theta^2 \left(\frac{1}{400} + \frac{\frac{1}{2}\Theta}{200,000} \right) \right\};$$

and this is the expression we shall use, writing it first in the simpler form,

$$\dot{\theta} = C\theta \left\{ 267 + \theta + \frac{\theta^2}{400} \left(1 + \frac{\Theta}{1000} \right) \right\}. \quad . \quad . \quad . \quad (7)$$

Notice that θ occurs in this expression as a factor, so that it is really a cubic function of θ .

It is singular how near the constant term in these brackets is to the number 274. I suppose this is accidental; but at first sight it looked as if the rate of cooling for small excesses of temperature were proportional to the product of absolute temperature and ex-

cess, or as if the quotient $\frac{\dot{\theta}}{v\theta}$ would be constant. On this hypothesis, however, the constant a , twice the reciprocal of whose logarithm is the number which happens to be nearly 274, would vary with v_0 the temperature of the enclosure, which is contrary to Dulong's results. Indeed there seems no ground for the conjecture.

Applying the correction for the neglected terms of the series, as is done in (7), we may write the expansion (6) thus, writing α instead of $\log_e a$ for shortness ($\alpha = .0076$),

$$\alpha^\theta - 1 \simeq \alpha\theta \left\{ 1 + \frac{1}{2}\alpha\theta + \frac{1}{6}\alpha^2\theta^2 \left(1 + \frac{1}{8}\alpha\Theta \right) \right\}. \quad . \quad . \quad (7')$$

13. It remains now to show, from Tables of experimental results, to what amount of accuracy θ , multiplied by a quadratic

function of θ , will represent the observed rate of cooling of a body in a vacuum.

And first I will take the experiments of Narr* (see Wüllner, vol. iii. p. 254). The following Table contains the result of his experiments in a vacuum. The first column is the observed rate of cooling at the Centigrade temperature shown in the second column, the enclosure being at zero Centigrade. The third column contains the product of excess of temperature t and absolute temperature v , divided by the rate of cooling, to show how far this ratio is constant. These numbers are observed to decrease regularly, though slowly, and in a manner which has an obvious relation to the corresponding number in the preceding column; so that if twenty times that number be subtracted from each, the result will be very constant, as is shown in the last column.

\dot{t} .	t .	$(274+t) \frac{t}{\dot{t}}$.	$\frac{vt}{\dot{t}} - 20t$.
3.26	115°	13720	11420
3.11	110	13580	11380
2.80	100	13360	11360
2.49	90	13160	11360
2.18	80	12990	11390
1.88	70	12810	11410
1.73	65	12740	11440

Hence

$$\frac{vt}{\dot{t}} - 20t = \text{const} = 11400.$$

We may write this,

$$20\dot{t} = \frac{247+t}{570+t} \cdot t, \quad \dots \dots \dots (8')$$

or, approximately,

$$\dot{t} \simeq \frac{t}{11400} (274+t) \left(1 - \frac{t}{570}\right),$$

which is of the form of equation (7), namely the excess of temperature t multiplied by a quadratic factor. The numerical value of the constants do not, indeed, agree well with those of Dulong, especially in the fact of the sign of the coefficient of t^2 being negative; but this is hardly to be expected, as Narr seems to have undertaken his experiments with the object of upsetting Dulong's results. Narr's experiments, moreover, do not extend over any thing like the range of temperature that Dulong and Petit's did.

* Pogg. *Ann.* vol. cxlii.

14. If we apply the same process to the Table expressing the results of the latter experimenters in a vacuous enclosure at Centigrade zero, we shall find that the number +30 has to be used instead of -20; so that $\frac{vt}{t} + 30t$ is very tolerably constant, and equal to 18650 on the average, as is shown in the following abridged Table of Dulong's results. Considering that the range of temperature extends as high as 240°, the agreement is pretty good,

$\dot{\theta}$.	θ .	$\frac{(274+\theta)\theta}{\dot{\theta}}$.	$\frac{v\theta}{\dot{\theta}} + 30\theta$.
10.69	240°	11530	18730
7.40	200	12810	18800
4.89	160	14200	19000
3.02	120	15650	18050
1.74	80	16270	18670

Hence we may write

$$30\dot{\theta} = \frac{274 + \theta}{622 - \theta} \cdot \theta, \quad (8')$$

or, approximately,

$$\dot{\theta} = \frac{\theta}{18650} (274 + \theta) \left(1 + \frac{\theta}{622}\right),$$

which is the form of equation (7).

It may be hereafter convenient to know that an expression like (8') and (8'') is capable of representing the law of cooling in a vacuum with great accuracy, viz,

$$\dot{\theta} = C\theta \cdot \frac{A + \theta}{B - \theta}; \quad (8)$$

but for our present purpose I think the equation (7) will be the most convenient.

15. The agreement of equation (7), as it stands, with Dulong and Petit's results it is scarcely necessary to show by a Table, since the equation has been deduced by known approximation from their own statement which completely expressed them, and the value of the terms neglected for an excess of temperature so high as 240° is perfectly evident. Nevertheless I have made the calculation, and the values of the "constant" $\left(267 + \theta + \frac{6}{5} \cdot \frac{\theta^2}{400}\right) \frac{\theta}{\dot{\theta}}$, or $\frac{1}{C}$, corresponding to the successive excesses of temperature 240°, 200°, 160°, 120°, and 80°, are

15265, 15865, 16489, 17086, and 16827. Hence the discrepancy between equation (7) and experiment is not *great* even for temperatures so high as 240° ; while for a maximum temperature under 150° or so the discrepancy is practically *nil*.

[To be continued.]

XXXV. Notices respecting New Books.

Scientific Memoirs; being experimental Contributions to a Knowledge of Radiant Energy. By JOHN WILLIAM DRAPER, M.D., LL.D., &c. &c. New York: Harper and Brothers. 1878 (8vo, pp. 473).

DR. DRAPER here brings together the scattered memoirs and essays that he has written during the past forty years on subjects connected with Radiation and Radiant Energy. They are thirty in number, and for the most part are simply reprints; but in a few cases the original memoirs are condensed, and in one or two cases the article here given is the substance of a considerable number of detached articles. Most of them have already appeared in our pages; the earliest of them, on subjects relating to Photography, appeared in 1840. "I have endeavoured," the author tells us, "to reproduce these memoirs as they were originally published. When considerations of conciseness have obliged me to be contented with an abstract, it has always been so stated, and the place where the original may be found has been given. Sometimes, the circumstances seeming to call for it, additional matter has been introduced; but this has always been formally indicated under the title of *Notes*, or included in parentheses" (p. x).

It is probably known to our readers that Count Rumford made a donation to the American Academy of Arts and Science (similar to that which he made to the Royal Society) for rewarding discoveries and improvements relating to light and heat made in America. The Academy has been rather chary of bestowing its honours, and had only awarded its Rumford Medal four times before it made the award in 1875 to Dr. J. W. Draper "for his researches in Radiant Energy." This circumstance has determined the selection of articles in the present volume. It comprises the researches on which the Award was founded.

The President's statement of the grounds of the Award is given in the Appendix, and may be summarized as follows:—

- (a) Independent discovery of Moser's Images.
- (b) Measurement of the Intensity of the Chemical Action of Light by exposing to the Source of light a mixture of Equal Volumes of Chlorine and Oxygen.
- (c) Application of Daguerreotype process to taking portraits.
- (d) Application of ruled glasses and specula to produce Spectra for the study of the Chemical Action of light.
- (e) Investigation of the nature of the rays absorbed by growing plants in sunlight.

(f) Discussion of the Chemical Action of light and proof that rays of all wave-lengths are capable of producing chemical changes.

(g) Researches on the distribution of Heat in the Spectrum.

And, finally, an elaborate investigation, published in 1847, by which he established experimentally the following facts, which we will give in the words of the Award :—

“1. All solid substances, and probably liquids, become incandescent at the same temperature.

“2. The thermometric point at which substances become red-hot is about 977° Fahr.

“3. The spectrum of an incandescent solid is continuous ; it contains neither bright nor dark fixed lines.

“4. From common temperatures nearly up to 977° Fahr. the rays emitted by a solid are invisible. At that temperature they are red ; and the heat of the incandescing body being made continuously to increase, other rays are added increasing in refrangibility as the temperature rises.

“5. While the addition of rays, so much the more refrangible as the temperature is higher, is taking place, there is an increase in the intensity of those already existing.” The Award then proceeds as follows :— “Thirteen years afterwards Kirchhoff published his celebrated memoir on the relations between the coefficients of emission and absorption of bodies for light and heat, in which he established mathematically the same facts and announced them as new.”

We are, of course, aware that this is rather a burning question ; but whatever may be thought of the justice of these claims, there can be no doubt that the fact of their having been made on behalf of Dr. Draper by so distinguished a body as the American Academy of Arts and Science ought to be known, and that its judgment will receive at least respectful consideration whenever the early history of Spectroscopic Science comes to be written. And it is impossible not to draw attention to the fact in a notice, however brief, of Dr. Draper's volume ; for plainly one of the motives of its publication is to assert his claims to priority of discovery in regard to the points above quoted. In fact the four memoirs which bear directly on the subject of Spectrum Analysis are printed first in the volume, and are followed by a note in which Dr. Draper complains, though in very decorous language, that he has received considerably less than justice at the hands of M. Kirchhoff ; and by way of showing that he has tangible grounds for complaint, he makes the following quotation (p. 85) from M. Jamin's *Cours de Physique*, in which results that he had previously established are formally attributed to M. Kirchhoff.

“M. Kirchhoff has deduced the following important consequences :—

“Black bodies began to emit at 977° Fahr. red radiations, to which are added successively and continuously other rays of increasing refrangibility as the temperature rises.

"All substances begin to be red-hot at the same temperature in the same enclosure.

"The Spectrum of solids and liquids contains no fixed lines."*

Now it may be said with very little qualification that what is here attributed to M. Kirchhoff is to be found distinctly stated in the first memoir in the volume before us, which was published by Dr. Draper in 1847. By experimenting with a strip of platinum heated by the transmission of a current whose force could be regulated, he ascertained that the temperature at which red rays are first radiated is 977° Fahr. He also ascertained that platinum, brass, antimony, gas-carbon, and lead became incandescent at the same time with the iron barrel in which they were gradually heated, and that the apparent exceptions presented by chalk, marble, and fluor-spar were due to phosphorescence. By raising the temperature of the platinum wire and analyzing with a prism the light emitted, he proved that the length of its spectrum gradually increased with the temperature until at 2130° Fahr. the full spectrum of daylight was attained; and it is clear that he regarded the result thus obtained as being generally true. That the spectrum of the incandescent platinum contained no dark lines did indeed come out only incidentally in the course of the investigation: still it was not by any means a point seen but not observed; for, in consequence of observing it he resorted to a comparison of the spectra of incandescent platinum at different temperatures with the spectrum of daylight in order to determine their extent, instead of fixing their extent by the dark lines of the spectra themselves, which he had ascertained to be non-existent. On the whole the above statement breaks down at nearly every point. What is therein referred to M. Kirchhoff was certainly ascertained before by Dr. Draper. Whether Dr. Draper was the first person to observe all these points is a very different question, and one we would by no means pre-judge; indeed, without going beyond the limits of the first memoir, it is pretty plain that the temperature of incandescence was known with considerable accuracy before Dr. Draper's experiment with the platinum wire; and it certainly was believed (if not proved) that the temperature was the same for all bodies.

Geological Survey of Canada. Report of Progress for 1876-77.

A. R. C. SELWYN, F.R.S., F.G.S., Director. 8vo. Pp. 531.

Dawson Brothers: Montreal, 1878,

Besides the Introductory Report by the Director, this volume contains the following important Reports:—1. On Geological Exploration in British Columbia, chiefly on the Blackwater, Salmon, and Nechaco Rivers and François Lake; by G. M. Dawson; with coloured map and seven very suggestive plates of local scenery.

*The above quotation is, we presume, to be found on pp. 463, 464, vol. iii. ed. 1866. If so, it is not exactly a quotation, but is made up of parts of a much longer statement. We may also observe that Memoir I. of the present volume is not in all respects an exact verbal reprint of the Memoir published in our Journal for May 1847. This does not, however, affect the point at issue.

Phil. Mag. S. 5. Vol. 7. No. 42. March 1879. R

2. Reconnaissance of Leech River and Vicinity (auriferous); by G. M. Dawson. 3. The Mines and Minerals of British Columbia (Gold, Coal, Lignite, Iron, Silver, Copper, &c.); by G. M. Dawson. 4. Jurassic fossils from the Iltasyouca River, British Columbia; by J. F. Whiteaves. 5. Coal-fields of Vancouver and neighbouring islands, and the Tertiary rocks of Sooke Bay, &c.; by James Richardson; with coloured map and sections. 6. Geological Research north of Lake Huron and east of Lake Superior; by Robert Bell. 7. The Goderich Salt Region; by T. Sterry Hunt. 8. Geology of the Counties Renfrew, Pontiac, and Ottawa, with their iron-ores, apatite, and plumbago; by H. G. Vennor; with coloured map. 9. The Slate-formations and general Geology of Charlotte Co., New Brunswick; by G. F. Matthew. 10. Lower Carboniferous belt, including the Albertite and its shales &c., in New Brunswick; by L. W. Bailey and R. W. Ellis; with map and sections. 11. Geology of part of Cape Breton, with notices of the coals and metals (including gold) found in the vicinity; by Hugh Fletcher; with coloured map. 12. Additions to the Insect fauna of the Tertiary beds at Guesnel, British Columbia; by Samuel Scudder. 13 and 14. Notes on Rocks and Minerals; by B. J. Harrington and Christian Hoffmann.

A comprehensive Index completes this volume, which is full of important information, interesting in every branch of geological research—physiographic, stratigraphic, palæontographic, petrographic, and mineralogical,—so much so, indeed, that the mere list of main subjects alluded to above, must indicate to any one that veteran geologists will see many of the old classic regions of North-American geology greatly elucidated by modern research, and rising students will have to congratulate themselves on difficulties having been removed from their paths, and a very wide and clear field opened in many directions for their own researches. To the political and social economist the explanation and mapping of the soils, structure, and mineral products of the great Canadian Dominion is necessarily of immeasurable importance; and the good useful work brought to general comprehension in this Report is therefore an Imperial benefit.

Remarks on the Sedimentary Formations of New South Wales. Illustrated by References to other Provinces of Australia. By the Rev. W. B. CLARKE, M.A., F.R.S., F.G.S., &c. &c. Fourth Edition. 8vo. Pp. 165. Richards, Sydney; Trübner, London, 1878.

Well worthy of being recorded among the early race of Geologists, who worked out their views of the science as presented by the phenomena observed, and defined by the general knowledge with which they had enriched their natural genius, the late W. B. Clarke was one of those who were led by special taste to cultivate the natural-history sciences before schools and colleges provided means of definite instruction in these matters. His first geological observations were published in an early series of the Magazine of

Natural History. After his settling in Australia he followed his bent in Geology, and got an insight into the general structure of the eastern mountain-ranges and of the strata flanking them on the east; and he was one of the first to recognize the gold-drifts and auriferous rocks. The relative position and age of the Coal-measures there he also studied; and he philosophically concluded that certain plant-remains in the Australian coal did not necessarily give it a Jurassic age, or remove it from the Palæozoic series, because they resembled, or even were identical with, a fossil plant found in the Oolites of Yorkshire. The persistence of many low organisms, the wide extent and long-continued emigration of successions of similar creatures, as well as other considerations, strongly supported the veteran observer in his discussions with those who would draw equally definite demarcations for the extinct faunæ and floræ of Britain and for those of its antipodes. Of this and many other subjects, interesting to both the historian and the student of geology, this last edition of Mr. Clarke's condensed observations contains a complete account, involving, too, a considerable series of notes on the geological structure and history of other regions. It was finished on June 2, 1878, the eightieth birthday of the enthusiastic author, who had for many years given his best exertions to the elucidation of Australian geology, to its beneficial application to his fellow countrymen, and to the dissemination of a sound knowledge of his favourite science to all whom his words and writings could reach. He ceased from his labours before the year was out, respected and honoured, leaving behind him the legacy of good work, with its sure results in advancing the welfare of his fellow men.

XXXVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 143.]

Jan. 22, 1879.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On Community of Structure in Rocks of Dissimilar Origin." By Frank Rutley, Esq., F.G.S.

After alluding to the community in mineral constitution of certain rocks to which different names have been applied, and indicating the advisability of retaining some old terms in a provisional sense, questions relating to the causes of the angular and rounded characters of certain rock-constituents were discussed. The author then described some of the more common structural peculiarities met with in rocks of various origin, especial attention being directed to those in which microcrystalline, cryptocrystalline, or microfelsitic conditions have been either normally developed or superinduced; while other rocks were de-

scribed in which corresponding structure, sometimes coupled with a similar mineral constitution, may be met with. Difficulties attending the determination of the origin of some clastic rocks were also pointed out, and the value of certain structural characters in their diagnosis were mentioned. Assumptions as to the origin of some fragmentary rocks were shown to be undemonstrable in certain cases, although such assumptions often carried much probability with them. The resemblances presented by devitrified rhyolitic rocks, felstones, and feldspathic grits were dwelt upon at some length. The paper included a short structural classification of the constituents of rocks.

2. "Distribution of the Serpentine and associated Rocks, with their Metallic Ores, in Newfoundland." By Alexander Murray, Esq., C.M.G., F.G.S.

The author stated that no extensive display of serpentine is known in the Laurentian series in Newfoundland; nor is the existence of crystalline limestone of that age, with which serpentine is often associated, as yet well established. The Intermediate or Huronian system is singularly barren in lime, magnesian minerals, and mica, lime occurring almost exclusively as intersecting calcareous veins. Over all the known area of the system no masses of serpentine have been observed, and only one instance of the presence of a serpentinous mineral, which occurs in an intrusive mass intersecting the Intermediate system and disturbing the outcrop of the sandstones of the Primordial Silurian (Lingula Flags) at a place called "The Broad" of Tickel Harbour, Trinity Bay, where some steatite with some seams of asbestos were seen near the contact. Wherever a typical fossiliferous horizon could be established, the stratigraphical position of the fossils placed those of the Lévis age, or older, below the serpentines; while in all cases, where the types were of Hudson-River or newer date, they as invariably succeeded unconformably above. Instances of this unconformable relation were mentioned in which the upper formation was as late as the Devonian age. The stratigraphical and palæontological break between the Lévis and Trenton groups is here filled up by a metamorphic mass which, in part at least, may possibly represent the horizon of the Chazy group; and the great intrusive masses have been connected with, or the cause of, the metamorphic phenomena displayed.

XXXVII. *Intelligence and Miscellaneous Articles.*

ON THE ELECTROMAGNETIC THEORY OF THE REFLECTION AND REFRACTION OF LIGHT. BY GEORGE FRANCIS FITZGERALD, M.A., FELLOW OF TRINITY COLLEGE, DUBLIN.

THE media, at whose surfaces reflection and refraction are supposed to take place, are assumed to be nonconductors, and isotropic as regards magnetic inductive capacity. Some reasons are advanced

why the results should apply at least approximately to conductors. In the first part of the paper the media are not assumed to be isotropic as regards electrostatic inductive capacity; so that the results are generally applicable to reflection and refraction at the surfaces of crystals. I use the expression given by Professor J. Clerk Maxwell in his 'Electricity and Magnetism,' vol. ii. part 4, chap. 11, for the electrostatic and electrokinetic energy of such media. By assuming three quantities ξ , η , and ζ , such that, t representing time, $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}$, and $\frac{d\zeta}{dt}$ are the components of the magnetic force at any point, I have thrown these expressions for the electrostatic and electrokinetic energy of a medium into the same forms as M'Cullagh assumed to represent the potential and kinetic energy of the æther, in "An Essay towards a Dynamical Theory of Crystalline Reflection and Refraction," published in vol. xxi. of the Transactions of the Royal Irish Academy. Following a slightly different line from his, I obtain by a quaternion and accompanying Cartesian analysis, the same results as to wave-propagation, reflection, and refraction as those obtained by M'Cullagh, and which he developed into the beautiful theorem of the polar plane. Of course, the resulting laws of wave-propagation agree with those obtained by Professor Maxwell from the same equations by a somewhat different method. For isotropic media, the ordinary laws of reflection and refraction are obtained, and the well-known expressions for the amplitudes of the reflected and refracted rays.

In the second part of the paper I consider the case of reflection at the surface of a magnetized medium, adopting the expressions Professor J. Clerk Maxwell has assumed in 'Electricity and Magnetism,' vol. ii. part 4, § 824, to express the kinetic energy of such a medium. From this, following the same line as before, I have deduced the following equations to represent the superficial conditions. In them ξ , η , ζ have the same meaning as before, and the axes are x in the intersections of the plane of incidence and the surface, y in the surface, and z normal to it; α , β , γ are the components of the strength of the vortex that Professor Maxwell assumes, and

$$\frac{d}{dh} = \frac{d}{dx} + \beta \frac{d}{dy} + \gamma \frac{d}{dz},$$

which, with these axes, reduces to

$$\alpha \frac{d}{dx} + \gamma \frac{d}{dz};$$

K and K_1 are the electrostatic inductive capacities of the two media in contact; and the quantities referring to one of these which is supposed to be non-magnetic are distinguished by the suffix $_1$; C is a constant, on which the power of the medium to rotate the plane of polarization of light depends.

$$\frac{d\xi_1}{dz_1} - \frac{d\zeta_1}{dx_1} = \frac{K_1}{K} \left(\frac{d\xi}{dz} - \frac{d\zeta}{dx} \right) - 4\pi CK_1 \left\{ \gamma \frac{d^2\eta}{dz dt} + \frac{d^2\eta}{dh dt} \right\}$$

$$\frac{d\eta_1}{dz_1} = \frac{K_1}{K} \cdot \frac{d\eta}{dz} + 4\pi CK_1 \left\{ \gamma \frac{d}{dt} \left(\frac{d\xi}{dz} - \frac{d\zeta}{dx} \right) + \frac{d^2\xi}{dh dt} \right\}.$$

As these are unchanged by a simultaneous alteration of the signs of η and C , I show that the method adopted in my former paper on Magnetic Reflection, in the 'Proceedings of the Royal Society' for 1876, No. 176, is justified, and that it is legitimate to consider an incident plane-polarized ray as composed of two oppositely circularly polarized rays, each of which is reflected according to its own laws. From these I further deduce that, when the magnetization of the medium is all in the direction of η , there is no effect on reflection or refraction produced by it. I consider next the cases of the magnetization being all normal to the surface, and all in the surface and the plane of incidence, and obtain the following result:—When the incident ray is plane polarized, and the plane of polarization is either in or perpendicular to the plane of incidence, the effect of magnetization is to introduce a component into the reflected ray perpendicular to the original plane of polarization, whose amplitude, c , is given in the several cases by the following equations, in which i is the angle of incidence, and r of reflection, and k a small constant depending principally on C and the intensity of the incident ray 1. When the magnetization is normal to the reflecting surface. If the incident ray be polarized in the plane of incidence,

$$c = k \cdot \frac{(1 + \cos^2 r) \sin^2 i \sin 2i}{\sin r \cdot \sin^2(i+r) \cdot \cos(i-r)}.$$

If it be polarized in a plane perpendicular to the plane of incidence,

$$c = k \frac{\cos^2 r \cdot \sin^2 i \sin 2i}{\sin r \cdot \sin^2(i+r) \cdot \cos(i-r)}.$$

2. When the magnetization is parallel to the intersection of the surface and the plane of incidence, and the plane of polarization of the incident ray is either in or perpendicular to the plane of incidence,

$$c = k \frac{\cos r \sin^2 i \sin 2i}{\sin^2(i+r) \cos(i-r)}.$$

This vanishes at the grazing and normal incidences, and, in the case of iron, attains a maximum at about the angle of incidence $i = 63^\circ 20'$.

I do not obtain any change of phase by reflection in any case; and this was to be expected, as this change of phase probably depends on the nature of the change from one medium to another, which, following M'Cullagh, I have uniformly assumed to be abrupt. Apart from this question of change of phase, my results conform completely to Mr. Kerr's beautiful experiments on the reflection of light from the pole of a magnet, as published in the *Philosophical Magazine* for May 1877 and March 1878.—*Proceedings of the Royal Society*, January 9, 1879.

ON THE VELOCITY OF VERY LOUD SOUNDS. BY WILLIAM W. JACQUES, FELLOW OF THE JOHNS HOPKINS UNIVERSITY.

It is very well known that the velocity of a musical sound is, within very wide limits, sensibly independent of its intensity and of its pitch. The experimental proof of this is that a piece of music, played by a military band at a considerable distance, comes to the ear of the observer with its harmony entirely undisturbed.

A consideration of the theory of the propagation of a musical sound, too, shows that for sounds such as we ordinarily hear, in which the change of density from the rarefied to the condensed portion of the wave is small compared with the density of the undisturbed air, the velocity should be independent both of the intensity and the pitch.

When, however, we come to the consideration of a loud and sharp shock or explosion, in which the disturbances are very violent and abrupt, we cannot be at all sure that the changes of density are negligibly small, and hence that the velocity of sound for such cases would be a constant.

So little is known of the conditions in the case of the formation and propagation of sound from a centre of explosion, and the mathematical considerations of such conditions as we may presume are so difficult, that we must look almost entirely to experiment for our knowledge of the propagation of very loud sounds. But our experimental evidence on this point is very limited. Nearly all of the experiments that have been made upon the velocity of sound have been made with cannon, and have not agreed remarkably well with each other; nor have the thermodynamic quantities calculated from them, on the supposition that the velocity is identical with that of a musical sound, agreed very well with the values of the same quantities determined by other methods. But we cannot say whether these errors are due to the character of the sound or to other causes.

The very short interval between the flash and the report of a stroke of lightning, even when it takes place at a considerable distance, has been instanced* as a proof of the greater velocity of very loud sounds; but, so far as the writer is aware, this has not yet been reduced to experiment.

The experiments of Regnault† in water-pipes showed that the velocity of a pistol-report became slightly less each time that it was reflected along the pipe; but the change was very small, and its cause is doubtful.

The following paper contains an account of some automatic measurements of the velocity of sound in the immediate vicinity of a cannon. The results show that the velocity near a cannon is considerably different from that at a distance, and point out a

* Earnshaw, *Phil. Mag.* 1860.

† Regnault's 'Memoirs.'

considerable error that has been introduced into the most important measurement of this quantity.

The experiments were made at the United-States Arsenal in Watertown, Mass.

The method used was an automatic measurement of the velocity at different distances (varying from 10 to 110 feet) from the mouth of the cannon, by means of a series of membranes* electrically connected with a chronograph.

In the midst of a large level field was placed a six-pound brass field-piece. In the rear of this, at distances of 10, 30, 50, 70, 90, and 110 feet from the mouth of the cannon, were placed the membranes, elevated about 3 feet above the ground. These membranes consisted each of a hoop 9 inches in diameter, over which was stretched a sheet of thin rubber. To the centre of the membrane, and on the side towards the cannon, was attached a very small shelf of polished brass. Upon this rested one end of a delicate steel spring, the other end being fixed to an independent support.

The wire that brought the current of electricity from the chronograph-house was connected with the spring; and from the shelf a second wire returned to the chronograph. When the spring rested upon the shelf the circuit was closed; the passage of the sound-wave, however, would move the membrane, and break the circuit, causing a register on the chronograph. When the spring fell it rested upon a contact-point, from which a wire ran to the next membrane of the series; so that the circuit, immediately after being broken at the first membrane, was made again through the second before the sound-wave reached it. In this way the current could be transferred to all the membranes of the series, and the successive breakings and makings of contact, as the sound-wave passed each one, could be registered on a chronograph placed at a distance.

The chronograph used was of the Schultz form, and consisted essentially of a rapidly and uniformly revolving cylinder of silver, covered with lampblack, which was made one pole of the secondary coil of an inductorium, the primary coil of which was in circuit with the membranes. The other pole of the secondary coil was a fine metal point brought very near to the surface of the cylinder. When the primary circuit was broken or completed at the membranes, a spark passed between the metal point and the cylinder, and made a fine dot in the lampblack. By the side of the point was an electrical tuning-fork, which traced a sinuous curve of times on the lampblackened surface of the cylinder. The time could thus be measured to $\cdot 00001$ of a second. All that was necessary then for the experiment was to choose a moment when the air was as nearly as possible at rest, and then, the membranes being in order, to start the chronograph and fire the gun. The distances between the membranes were then accurately measured, the times of passage between successive membranes determined from the chronograph,

* Regnault used membranes, though unlike these, in his water-pipe experiments.

and the temperatures read off from thermometers placed at each membrane.

The experiment was many times repeated with the membranes interchanged, with different velocities and parts of the chronograph-cylinder, and with other precautions, to prevent possible errors, but always with the same result. It was found that immediately in the rear of the cannon the velocity of sound was less than at a distance, but that going further and further from the cannon *the velocity of sound rose to a maximum considerably above the ordinary velocity, and then fell gradually to about the velocity usually received.*

In order to determine whether the first low velocities were due, as was supposed, to the retarding influence of the bodily motion of the air around the cannon, it was pointed at right angles to its first position, when it was found that the maximum velocity came nearer to the cannon. Had the cannon been turned in the direction of the line of membranes, the retardation would probably have become an acceleration. The experiment was, however, of course impracticable. That this apparent retardation was not due to the difference in time of action of the membranes (due to a variation of the force of the wave) is evident both from the very slight force required in either case, and from the fact that the variation noticed is in the wrong direction.

The charge of powder was considerably varied; and the heaviest charges, of course, caused the greatest deviation from the ordinary velocity.

The successive series of experiments, owing to differences in the charge and in the loading, gave different values of the velocity at any one place; but the facts above stated always remained the same.

Accordingly each series represents the condition of things better than the mean of several, and I have here given a table of three of the best series.

The first column represents the distance from the mouth of the cannon, the second the values of the corresponding velocities in the rear of the cannon when the charge was one and a half pounds, the third when the charge was reduced to half a pound, and the last when the cannon was pointed at right angles to the line of membranes.

Interval.	Velocities reduced to 0° C.		Side of cannon.
	Rear of cannon. 1½ lb.	½ lb.	
10- 30 feet.	1076 feet.		
30- 50 "	1187 "	1032	1067
50- 70 "	1240 "	1091	1162
70- 90 "	1267 "	1120	1201
90-110 "	1262 "	1114	1188

The conclusions that we may draw from these experiments are:—1. That the velocity of sound is a function of its intensity. 2. That experiments upon the velocity of sound in which a cannon is used contain an error, probably due to the bodily motion of the

air near the cannon. Evidently a musical sound of low intensity must be used for a correct determination of the velocity of sound. —Silliman's *American Journal*, February 1879.

RESEARCHES ON BELL'S TELEPHONE. BY HENRI DUFOUR.

The principles on which the construction of Bell's telephone is based are direct consequences of the phenomena of induction and electromagnetism; and from theoretical considerations all that passes in that instrument can be foreseen. When it is employed we are struck, on the one hand, with the minuteness of the vibratory motions necessary to produce magnetic modifications of the magnet and the induction resulting from it, and, on the other, with the relatively great intensity of the sounds produced.

It seemed to me that it would be interesting to verify upon a few instruments the principal phenomena which theory enables us to foresee, and to seek out some of the causes which may modify them.

The instruments employed were constructed by M. J. Cauderay, at Lausanne. The mean length of the magnet was 127 millims., the thickness of the vibrating plate from 0.159 to 0.175 millim. The induction-coil contained 46 metres of wire of 0.3 millim. thickness.

Intensity of the Currents.—The maximum intensity observable is obtained by pressing on the vibrating plate so as to bring it into contact with the soft-iron termination of the magnet; the displacement it thus undergoes is about 1 millim., and produced a deflection of $7-8^{\circ}$ upon the galvanometer which I used. An equal deflection in the opposite direction is observed when the plate resumes its initial position.

The movement of the plate towards the magnet produced an inverse induced current in the three instruments which I tried, the pole of the magnet being in fact behind the coil through which passed the cylinder of soft iron.

For the purpose of knowing if the two currents, direct and inverse, possess an appreciable difference of intensity, the wires of the telephone were put into communication with two carbon electrodes dipping in water, and which could be connected with the galvanometer by means of a commutator. A great number of vibrations of the plate were produced, so that a series of induced currents, direct and inverse, traversed the liquid; the electrodes connected with the galvanometer gave no polarization-current. From this we may conclude that the difference of intensity of the two currents is very slight. In the construction of the telephone, therefore, no account is to be taken of the action which this difference may in time exert upon the magnetization of the bar.

Two of the instruments employed had poles of opposite names submitted to the action of the coil; and when joined they worked as well as those which are symmetric.

Intensity of the Magnetism.—The variations of intensity of the magnetism were ascertained in the following manner:—The north

pole of the magnet of a Weber's magnetometer was submitted to the simultaneous action of the south pole (surrounded by the coil) of a telephone A and the south pole of a magnet; these two instruments were placed so that the bar of the magnetometer was in equilibrium between them. A second telephone, B, was in communication with A. The movements of the mirror of the magnetometer were observed by the ordinary method of reflection of the divisions of a rule in the field of a telescope.

A pressure exerted upon the plate of B permitted a slight displacement of the magnet to be ascertained; but the movement was too small to be measured; its direction was always that which the theory caused to be foreseen.

Vibrations of the Plate.—Some attempts were made to determine the vibrations of the plate. The first method employed consisted in transmitting the vibrations to a gas-flame. For this purpose the wide-mouthed bell of the telephone was replaced by a cylindrical one of small capacity. A cork, pierced with two holes through which passed two kneed tubes of glass, bounded within the cylinder a sort of little chamber comprised between the front face of the vibrating plate and the hind face of the cork. The illuminating-gas entered through the first tube, and issued, forming a small flame, at the extremity of the tapering second tube; so that the whole constituted something analogous to the manometric capsules which M. König places upon the pipes.

Every vibration of the plate is betrayed by a movement of the flame when the induced currents employed are those produced by a small Dubois-Reymond coil, even when the exterior coil is at 2 centims. from the extremity of the inducing coil. The currents produced by the voice in a second telephone cause no variation in the height of the flame.

The result was equally negative when a small mirror was borne on a kneed lever with its end resting on the vibrating plate. A ray of light reflected by the mirror did not appear to be displaced under the influence of the vibrations produced by the voice.

Finally, I tried to produce coloured rings between the vibrating plate and a lens placed upon it. For this a very thin piece of glass (Deckgläschen) was placed upon the vibrating plate, in contact with the slightly convex lower face of a lens. The sounds were transmitted by the instrument, although weakened. The coloured rings were observed through a telescope furnished with a reticule.

The displacement of a bright ring to the following dark one is produced by a difference in the thickness of the stratum of air equal to a quarter of a wave-length; that is to say, a change in the position of a yellow ring will be ascertained for about 0.000143 millim. displacement of the plate. This displacement is manifested by a diminution in the distinctness of the rings, which oscillate about their normal position. The displacements are observed very distinctly by employing the induced currents of a Dubois-Reymond coil; but it has not been possible to verify them for the currents produced by the voice.

Having heard it said that two telephones the localities of which

have very different temperatures do not work well, I desired to put the matter to the test by direct experiment. One of the instruments was left during several hours exposed to a temperature of -18° , while the other passed the same time in an enclosure heated to 40° C. The two instruments, put in communication, transmitted speech perfectly.

As soon as the telephone was employed on the telegraph-lines, the action was remarked which is exerted upon the instrument by the currents used to work the Morse apparatus, and passing in wires near that which connects the two telephones. This action is attributable to an induction-phenomenon, to a deflection, or perhaps to both causes combined. I have tried directly at what distance an intermittent current can produce an induced current appreciable with the telephone.

Two copper wires, perfectly insulated, were stretched parallel over a length of 15.2 metres, and at distances varying between 15, 35, and 45 centimetres. One of the wires joined the pile and the manipulator with the receiver of a Morse apparatus; the earth-line was formed by the gas-pipes. The two extremities of the other wire communicated directly with the telephone. The current employed produced a deflection of 60° on a telegraph-compass. Under these conditions all the motions of the manipulator were distinctly perceived; and I am persuaded that a telegraphist would have understood the signs produced by the manipulator, even when the distance between the two wires was 45 centimetres.

It may hence be concluded, therefore, that on telegraph-lines the noise heard in the telephone when a message traverses a neighbouring wire may be attributed, at least in part, to induced currents. This experiment may have a certain interest in the lecture-room, to show at what distance an induced current can be produced. In this respect the telephone is much more sensitive than the galvanometer.—*Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles*, No. 1, 1879, pp. 91–95.

HARMONIC ORBITS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

M. Th. von Oppolzer's "Vulcan"-orbit (*Comptes Rendus*, Jan. 6, 1879) represents another of my predicted harmonic orbits:—

	Distance.	Time.
Von Oppolzer	123	58.8 days.
Chase, prediction	120	58.1 „

This leaves only one "missing link" in my triple series of principal harmonics, extending from α Centauri to the Sun. There are many secondary harmonics, indicating possible asteroidal positions. One of these has been confirmed by Mouchez's second Watson orbit.

Faithfully yours,

Haverford College, Pennsylvania
February 10, 1879.

PLINY E. CHASE.

THE
LONDON, EDINBURGH, AND DUBLIN
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AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

APRIL 1879.

XXXVIII. *The Existence of the Luminiferous Ether.* By
ERNEST H. COOK, B.Sc., A.R.C.S., *Lecturer on Physics at
the Bristol Trade and Mining School**.

THE enormous velocity with which the motion producing light is propagated through space induced the authors of the undulatory theory to seek about for some medium capable of transmitting the vibratory movement. Applying the known laws of the propagation of sound, which is also a vibratory movement, this medium must possess enormously high elasticity and extremely small density. Such a medium is the luminiferous ether. This substance fills *all* space, and is imprisoned between the atoms of *all* bodies. The vibrations of the atoms of luminous bodies are communicated to the ether, and by it transmitted in all directions. Each particle of the ether makes a small movement to and fro; but the whole mass is thrown into wave-like motions. The elasticity or density of the ether in free space is different from that of the same ether when imprisoned by the molecules of material bodies. Thus in a refracting body like glass, the elasticity of the ether is less (or its density is greater) than in air, and the elasticity in air is less than in a vacuum. We therefore find that the velocity of light in glass is less than it is in air, and is less in air than it is in a vacuum. Moreover in most crystalline substances the elasticity of the ether is different in different planes; and we find that light traverses such substances with different velocities in different directions. The explanation

* Communicated by the Author.

Phil. Mag. S. 5. Vol. 7. No. 43. April 1879.

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given of this is that the different grouping of the molecules along certain lines in the crystal determines a different arrangement of the ether particles along these lines also. The existence of this ether was, and is, considered of such importance by the supporters of the undulatory theory, that we find attempts being made to determine experimentally some of its properties. Thus Fizeau has arrived at the conclusion that a moving body drags part of the ether along with it in its motion. Stokes accounts for aberration by attributing to the ether the properties of an elastic solid. In fact, all our philosophers accept without reservation the material existence of the luminiferous ether. It is impossible to fail to note the analogy between some of the propositions of this theory and those of the two-fluid theories of Magnetism and Electricity. These fluids also are imponderable, invisible, yet all-pervading. They interpose themselves between the molecules of bodies, and are rendered evident to us only by the effects they produce when treated in certain ways. If we accept all these theories, we must assume that between the molecules of every unfortunate body we have five distinct fluids—two magnetic, two electric, and the ether. Why do we not discard the four and retain the ether? and attribute to it some other properties which would enable it to perform the functions of the magnetic and electric fluids? We should then only have to imagine the existence of one hypothetical medium. But as an effort of the imagination, the invention of five hypothetical fluids is as easy as the invention of one.

In order, then, to obtain for light the enormous velocity which experiment has shown it to possess, the supporters of the undulatory theory have boldly filled all space with a substance which, conforming to the equation $v = \sqrt{\frac{e}{d}}$, possesses very great elasticity and very little density. The velocity with which light travels through air we may take to be 185,000 miles per second. Sound, let us suppose, travels at 1100 feet per second. Light therefore travels 888,000 times as fast through air as sound does. To find what proportion exists between the density and elasticity of the air and ether, let us suppose the velocity of sound through air to be equal to 1000 feet per second (in this calculation the correct velocity is, of course, 916 feet per second). Then we have

elast. of air : elast. of ether :: $1000^2 : (185,000 \times 5280)^2$,
 assuming the density to remain constant. This gives us 954,138,240,000 as the number of times that the elasticity of the ether is greater than that of the air, assuming the density

to remain constant. If we assume the elasticity to remain constant, we have that the density of the ether must be this number of times less than the density of the air. The necessity for the existence of the ether therefore is, that we require a body whose elasticity shall be this number of times greater, or whose density shall be this number of times less, than that of the air. It is hardly necessary to add that we know of no such body in nature; and since we cannot conceive of motion without having something moved, we invent the hypothetical substance ether, which shall be the vehicle of our wave-motion. But have we not been too eager to invent? have we thoroughly satisfied ourselves that matter itself (*i. e.* ordinary matter) is incapable of transmitting the vibratory movement? In attempting to answer these questions, we have first to show that the theory of the constitution of the luminiferous ether as at present held is untenable; and, secondly, we must endeavour to show that the particles of matter themselves are capable of taking up and transmitting the wave-motion.

Difficulties in the Conception of the Ether.

The difficulties which one meets with in the belief in the existence of this substance may be divided into two classes—those founded on theoretical considerations, and those founded on experimental evidence. We will first consider the former.

In the preceding portion we have stated that the ether must possess enormous elasticity and very little density. This is the view usually accepted: thus Tyndall says, “it is assumed to be of both extreme elasticity and of extreme tenuity.” Now, if this be the case, it of course follows that this ether will be distributed in space in the same way that matter is distributed; viz. it will accumulate around the celestial bodies, and the greater the mass of the body the greater the atmosphere (if we may use the term) of ether surrounding it. We ought, therefore, to find that a gradual increase of refractive power occurred as we approached a celestial body. In the comparatively few cases in which this can be tested it is found to be the case; but it is, I believe, universally attributed to the influence of the atmosphere. But we must consider this tenuity to be so great that it is impossible for us to recognize it by any of our balances. For if we carefully weigh a piece of a transparent substance, such as glass, and then grind it to powder (in which process we must liberate some of the ether which is held between the molecules), and weigh the powder, we obtain the same weight as before, showing that the ether we have lost had not sufficient weight to affect the balance. Some of the upholders of the undulatory theory, however, take

a different view: thus Sir John Herschel says, "though we suppose the ethereal molecules to possess inertia, we cannot suppose them affected by the force of gravitation." If this be the case, this hypothetical medium has no analogue in nature; it is a substance of which we can form no notion, as it is impossible to conceive a body possessing moving force but no weight. Are we not inventing too much when we endow a hypothetical substance with impossible properties? It is curious to observe how the same philosopher, in advancing arguments against the corpuscular theory, says, "This is one of the many weak points of the theory. It runs counter to the only analogy which the observation of nature furnishes"*. Yet eight pages further on he endows the ether with a property which causes it to be like no other substance in nature! Again, if the ether has no density, it is not necessary for us to assume it to possess a high elasticity; for any value given to the elasti-

city will fulfil the conditions of the equation $v = \sqrt{\frac{e}{d}}$. In fact, the equation has no meaning if v , e , or $d = 0$. We have thus arrived at these conclusions:—first, if we suppose the ether possesses weight, we ought to find an increase in refracting power near large masses; and, secondly, if we suppose the ether to be unaffected by gravitation, then it is a body which bears no resemblance to any other body we know of.

Again, the ether is supposed to pervade all bodies, to interpose itself between the molecules, and, moreover, to be affected by the grouping of these molecules. When light passes from one body into another, it does so by throwing the ether contained in that body into vibration. Refraction occurs because the elasticity of the ether in the second body is different from the elasticity of the ether in the first. But why is this elasticity different? We are told because the molecular arrangement is different in one body to what it is in another; but are we to suppose that the proximity of the ether to different modes of molecular grouping causes that ether to be of different elasticity? If this be so, we have to attribute to the ether a property which is peculiar to it, viz. that of having its elasticity altered by its proximity to different molecular groupings. If we, to avoid this conclusion, suppose that it is the density of the ether which undergoes change, then we again make a departure from all analogies. We know of no substance whose density is altered by the mere presence of another body.

Nor does it appear that we are better off if we assume the ether to be imprisoned between the molecules. For in this

* Familiar Lectures, p. 269.

case, although by the compression we do increase the density, yet we increase the elasticity in the same proportion, and consequently the velocity remains unchanged. Also, if we adopt this view, we must have the entire surface of a body composed of its own molecules: there must be no spaces; or the imprisoned ether would escape and assume the elasticity and density of the ether of the surrounding body. We thus arrive at the conclusion that, for refraction to be accounted for, we must assume that the elasticity of the ether is different in different bodies, and that this difference is due to the proximity of the ether particles to the different molecular arrangements of bodies. In this assumption, it is needless to state, we make a departure from all known facts. We know of no substance whose elasticity is altered by the proximity of another body. Slightly altering the words of Sir John Herschel, if we accept this explanation we are running counter to all the analogies which the observation of nature affords.

Again, although the ether in a certain sense is a most powerful substance, capable of entering into all bodies, and of vibrating with enormous velocity, yet chemically it is a most inert substance. In no way can we cause it to chemically combine with any other body. Although in intimate connexion with the molecules and atoms of all, it chemically affects none. In this property, also, our hypothetical medium is peculiar; no substance in nature refuses to combine with some one or more other substances.

The difficulties which one experiences in accepting the ether, owing to certain experiments, are varied and numerous. We will consider some of these. A common experiment in acoustics is to place an alarm under an exhausted receiver and to receive no sound when the air is withdrawn. This simple experiment is difficult to explain; for we must remember that, although the air is withdrawn, the ether remains. Why does this ether not take up the vibrations of the sounding body and transmit them? It cannot be because the vibrations producing sound are too slow; for an unlimited elastic medium is capable of vibrating in any way. The water of the ocean transmits a long rolling wave as readily as the minutest ripple, and the minutest ripple as easily as the shortest sound-wave. It will not suffice to say that the ether does take up the vibration and carry it on to the sides of the receiver, but that here it is unable to throw the molecules of the glass into vibration, and hence the sound cannot reach the external air, because according to theory the ether is contained in the glass. There is, in fact, no break in the line of communication of the ether particles outside with those in the inside of the receiver. What prevents the vibrations being taken up and transmitted?

Another experiment which seems difficult of explanation is that it is impossible to cause the electric discharge to occur in a perfect vacuum. Ether is present in this case also ; yet this ether is unable to transmit the electricity. Whatever this electricity may be, it is certainly something which is much like light and heat, it is therefore probably molecular motion of some kind. If molecular motion, our ether ought to be able to transmit it ; as it does not do so we are left to choose between two alternatives—either to say electricity is not due to molecular motion, or that no substance of the nature of ether is present. Evidence seems to be accumulating to show that electricity is due to molecular motion ; and we must therefore conclude that no ether is present.

Thirdly, let us consider Faraday's experiment with the "heavy glass." When a polarized ray is passed along the axis of a prism of a transparent substance which under ordinary conditions is optically inactive, and the prism placed in the magnetic field, then the substance becomes able to turn the plane of polarization. Thus, if a polarized ray is sent through a prism of this glass which is placed between the poles of an electromagnet, the plane of polarization is immediately turned. The reason assigned for this peculiar action by Faraday is that the magnet has caused a temporary difference in the molecular constitution of the substance ; and he finds that any cause which impedes the development of this power of rotation also impedes molecular displacement. But Faraday does not commit himself to the statement that this alteration of molecular grouping causes an alteration in the constitution of the ether of such a character as to cause it to vibrate in a particular plane. He simply states that a molecular rearrangement has produced the effect, and evidently considers this a sufficient explanation.

Two observations recently made are also difficult of explanation on the ethereal theory, viz. the increase of the electrical resistance of selenium by its exposure to light, and Dr. Kerr's experiments with the light reflected from the polished pole of a magnet (see *Phil. Mag.* May 1877 and March 1878). In the first of these we find that the vibratory movement of light affects the material particles of matter ; for we believe that the particles of the body are alone concerned in the transmission of electricity. This difficulty may be overcome by assuming the ether to be the medium for the conduction of electricity as it is of light ; but up to the present, I believe, this theory has not been propounded. Dr. Kerr finds that the light reflected from the polished pole of a magnet is polarized. We must therefore suppose that the ether is susceptible of magnetic influence. May I ask if it contains the two magnetic

fluids? If, however, we consider the particles of the air concerned in the propagation of light, we have to make no fresh assumption, as we already know nitrogen to be very feebly diamagnetic and oxygen paramagnetic. The particles of these gases are affected by contiguity to the pole of the magnet; and this kind of affection is such as to cause the molecules to vibrate in certain planes: hence the light is polarized.

Finally, let us look at the explanation offered by the ether theory to two of the chemical actions of light as representatives of most of the chemical effects of light. We will take the reduction of silver salts and the decomposition of CO_2 into its elements by the action of light on the chlorophyll of plants. Scheele discovered that when chloride of silver was exposed to light, a black powder, insoluble in nitric acid, was formed; and at the same time free hydrochloric acid was produced. The black powder he concluded to be metallic silver. He also made the important discovery that the violet rays were far more active in producing this reduction than the other rays. Here, then, we have three distinct results to account for, viz.:—1st, the production of the black powder; 2nd, the formation of hydrochloric acid; and, 3rd, the superior power of the violet rays. The ether theory says that the vibration of the ether shakes asunder the bond of chemical union; that is, the vibrations of the medium in which the molecules of the argentic chloride are imbedded causes the separation of the atoms of the molecules. This explanation seems at once insufficient and unnecessary. It is insufficient for this reason: it does not seem probable that the vibrations of an intermolecular medium should be able to cause an atomic separation. It is quite possible to conceive that the molecules are caused to vibrate by the vibrations of the surrounding ether; but that the individual atoms of these molecules are caused to vibrate, and *that with different velocities, or otherwise no separation can occur*, seems improbable. That this vibration of the atoms may occur it is necessary to assume that the ether is interatomic instead of intermolecular. If we make this additional assumption, we stretch to the point of breaking an already “elastic” theory, and we render much more difficult of explanation the combination of bodies produced by the action of light. The above explanation seems unnecessary, because we have only to assume that the atoms of bodies are capable of vibrating at a great velocity to account for these experiments in a simple and most satisfactory manner. The latter portion of this paper is devoted to the working-out of this assumption.

Continuing our investigation of this experiment further, we notice that colourless chloride of silver has been converted

into a black substance. A body is colourless because the power of the ether between its molecules to vibrate is the same as the power of the ether in the surrounding medium to vibrate. A body is black because its ether takes up and retains all the vibrations which fall upon it. Thus the vibration of the ether in the black substance must be more powerful than in the colourless. (This also follows because energy cannot be destroyed by the black substance.) Here, then, the vibration of the ether in the colourless chloride of silver has produced a substance containing ether particles vibrating with more energy. In other words, the energy of the vibration in the argentic chloride has been used up in first shaking the atom of silver from the atom of chlorine, and also in causing the ether in the silver and the ether in the chlorine to vibrate. According to the conservation of energy, these energies should be equal. But we find that there is more energy in the black silver than in the chloride; and consequently we have inequality.

The reason assigned for the superior energy of the violet rays is simple and satisfactory if we admit the previous assumptions. It is that in consequence of their more rapid vibration they are more energetic and thus capable of doing more work.

In the decomposition of carbonic anhydride effected by light in the presence of chlorophyll, we have another experiment which is difficult of explanation on the ether theory. The vibration of the ether of the carbonic anhydride is unable to effect the decomposition itself; but when brought *near* to the vibrating ether of the chlorophyll, the decomposition is effected. What is the nature of the action which here takes place? and what known action is analogous to it?

Stating briefly the difficulties in the conception of the ether, we have:—

(α) The want of any direct evidence.

(β) The fact that no ethereal condensation is observed around the celestial bodies.

(γ) The interposition of this substance between the molecules of bodies.

(δ) The nature of the action producing a difference of elasticity or density in this imprisoned ether.

(ϵ) The chemical inertness of the ether.

In addition to these we have from experimental evidence the following:—

(α) Inability of the ether to take up every species of vibration.

(β) Inability to transmit electricity.

(γ) Inability to explain circular polarization produced by magnetic action.

(δ) Incompleteness of explanation offered of the chemical effects of light.

Probably a more exhaustive survey will reveal other and greater difficulties; but these will be sufficient to show that the acceptance of the theory is beset with difficulties and makes such great calls upon our imaginations that it behoves the least sceptical to "pause and consider."

Now let us proceed with the second portion of our subject, to endeavour to show that the particles of matter themselves are capable of taking up and transmitting the wave-motion. Dalton considered all bodies to be composed of atoms, which atoms are all of the same size but of different weights. This difference in weight is expressed in the atomic weight of the elements. Atoms in the free state combine with each other and form molecules. About the absolute size or density of these atoms we know nothing, save that they are very very small. Recent advances in scientific theory have but extended Dalton's hypothesis. Thus, a high authority, stating the theory at present held of the constitution of bodies, says:—

"All bodies consist of a finite number of small parts called molecules. Every molecule consists of a definite quantity of matter, which is exactly the same for all the molecules of the same substance. . . . A molecule may consist of several distinct portions of matter held together by chemical bonds, and may be set in vibration, rotation, or any other kind of relative motion. . . . The molecules of all bodies are in a state of continual agitation. The hotter a body is, the more violently are its molecules agitated"* . Here, then, heat is produced by the vibration of the molecules of bodies. If the particles of a body are capable of such rapid vibration in the production of heat, why may they not be capable of taking up this vibration and transmitting it? It is more than probable that heat is thus transmitted. Why may not light be also thus transmitted? We only have a difference in quantity between the two, heat being produced by the less rapid and light by the more rapid vibrations. Nor is the difference so great between the vibration producing heat and that producing light. The maximum heating effect of the spectrum occurs at a point where the rate of vibration is about 400 millions of millions of vibrations per second; the maximum chemical effect occurs where the rate is about 800 millions of millions per second. As an effort of the intellect, it is as easy to endow matter with the ability to vibrate at the latter rate as at the former; and if

* Maxwell, *Theory of Heat*, p. 306.

it can vibrate at this rate (which seems to be admitted) why cannot it transmit this vibration?

It cannot transmit it because its density is too great and its elasticity too small we should be told. But if we reduce the size of the atom we reduce the weight of it, and we reduce the amount of energy necessary to throw it into vibration. In fact, if we reduce the size 954,138,240,000 times, we should reduce the weight and endow the atom with power of vibrating. It appears, then, that the ability of a body to take up vibratory motion depends upon the size of the atom; and, given that the atoms and molecules of bodies are sufficiently small, they are capable of transmitting light and heat. In order to compare the velocity with which light travels with that which sound travels, let us take a few examples. In air we have already seen that light travels with 888,500 times the velocity of sound. In water we know that sound travels at about 5000 feet per second. The index of refraction of water is, according to Brewster and Wollaston, 1.336; if the velocity of light in air is 185,000 miles per second, the velocity in water is 138,500. Thus the velocity of light in water is 146,300 times that of sound. The greatest velocity of sound in any substance is through iron at 100° C., in which it is 17,500 feet per second. The substance with the greatest refractive index is chromate of lead = nearly 3. The velocity of light is therefore less in this body than in any. Let us compare these two, and we find that the velocity of light in chromate of lead is *only* 17,620 times as great as the velocity of sound through iron. Now we have no doubt that the sound is transmitted through the iron by the vibration of its molecules, why may not the light be transmitted through the chromate of lead by the vibration of its molecules? Here, again, it is merely a question of degree; the one is a more rapid motion than the other. There is another point which must not be overlooked in comparing sound and light; and that is, that the vibrations of the molecules composing a light-wave oscillate in planes perpendicular to the direction of propagation of the wave, while those composing a sound-wave oscillate in the direction of propagation. This difference, it seems reasonable to suppose, will exercise an important influence on the relative velocities even in a homogeneous medium. It is evident that it must do so in a medium whose elasticity is different in the two planes, *i. e.* the plane of propagation and the plane of vibration.

And in thus assuming that the particles of matter themselves are capable of vibrating and propagating the undulatory movement of light, are we making too great a strain upon them? Certainly not. We can form no notion, even the most remote,

of the magnitude of these particles. The best microscopes will detect particles $\frac{1}{100,000}$ of an inch in diameter; yet "we are here dealing with infinitesimals, compared with which the test-objects of the microscope are literally immense"* . By means of the spectroscope we can detect $\frac{1}{200,000,000}$ of a grain of sodium; so that the atom of this metal must be smaller than this. Also, "the number of molecules in a cubic millimetre of atmospheric air is about a unit-eighteen (10^{18})"†—that is, one million billions! (A billion is a million times a million.) With a wave-length of $\frac{1}{5000}$ of a millimetre, we cease to have any luminous effect, but we still possess a faint photographic effect. We therefore see that the shortest waves in the spectrum are of immense length when compared with the size of the molecules of a body. With regard, then, to the size of the molecule, we can have no doubt that it is sufficiently minute to be fully able to oscillate and produce waves of the size of those of light. But can these molecules oscillate with sufficient rapidity? In an article on "Polarization Stress in Gases" (Phil. Mag. Dec. 1878), Mr. G. J. Stoney supplies data which will enable us to answer this question. At common temperatures the average velocity of the molecules of air may be taken as 500 metres per second. The molecules meet with so many encounters that the direction of the path of each is changed 10,000,000,000 times a second. We have, then, that in one movement the particle travels $\frac{1}{2,000,000}$ of a metre, or $\frac{1}{2000}$ of a millimetre; and it makes this movement in $\frac{1}{10,000,000,000}$ of a second. Now we have seen that the length of the wave of the extreme chemical ray is $\frac{1}{5000}$ of a millimetre; consequently we find that the molecule of air travels through a distance which is more than twice as long as the length of this particular wave in this fraction of a second. The time of one oscillation of the molecules composing the mean chemical rays may be taken as $\frac{1}{800,000,000,000}$ of a second. Thus, in 80 times as long as the time occupied by a molecule in one oscillation the molecule of air has travelled through a distance twice as long as that of the whole wave-length. The distance moved through by a wave would be underestimated at a million times the distance moved through by a molecule composing that wave; consequently we see that our air-particles move with a far higher velocity than that required by the shortest waves of the spectrum.

* Tyndall, 'Scientific Use of Imagination,' p. 25.

† Johnstone Stoney, Phil. Mag. December 1878.

With these considerations before us, what need is there to assume the existence of an all-pervading ether? The particles of ordinary matter are small enough, and can, nay do, vibrate at the requisite speed; why, then, are these particles not able to transmit the waves of light? Substituting for the luminiferous-ether theory this molecular theory, let us now see if the explanation of some of the difficulties of the former theory are satisfactorily accounted for. Before doing so, however, I will endeavour to answer two objections which it seems to me may be made against this theory. First, it may be asked, If light travels through bodies by the vibrations of its molecules, why is not the velocity of light through the body the same as the velocity of sound? In answering this we must bear in mind the differences between sound and light. The shortest wave of sound would be produced by 38000 vibrations per second, and would have a wave-length of about 9 millimetres, or 11250 times as great as the length of the longest wave of light. We have also to remember that the particles of a sound-wave oscillate in the direction of propagation, whilst those in a light-wave oscillate in planes perpendicular to that of propagation. Is it unreasonable to suppose, then, that so vast a commotion as that produced by sound *in the direction of propagation* should be retarded more than the minute disturbance produced by light in planes at right angles to this direction? Another objection which may be urged is, How do we account for the motion reaching us from the sun? We may do this in two ways: first, we may fill the space between the sun and earth with the luminiferous ether, and give this ether the property of non-miscibility with the atmosphere; or, secondly, we may assume the unlimited extent of our atmosphere. Either of these assumptions would be sufficient to account for the phenomena; and both have before been made.

Let us now see if these difficulties we have mentioned in regard to the conception of the ether are lessened if we consider the particles of matter to vibrate. It is evident that a condensation of matter does occur around the celestial bodies, and also that a gradual increase of the refractive power occurs as we approach large masses. We have here no difficulty in conceiving the cause of the difference in the refractive powers of bodies; it is simply due to the different density of the bodies and to the mode of grouping of the molecules interfering with perfect freedom of motion of these molecules. A glance at what may be termed the experimental difficulties given above will suffice to show the ability of this theory to satisfactorily account for these experiments.

Numerous experimental facts support this assumption in an

indirect manner. For instance, the greater the atomic weight of the substance the greater ought to be the refractive power or the less the velocity in the body. Unfortunately, however, this rule cannot be generally applied, because of other conditions which prevent the free motion of the molecule. But we know that in the gaseous condition the molecules are less hampered than in the liquid and solid states; the refractive indices of gases ought therefore to exhibit some increase with the density. The following are the indices of refraction and densities of the five simple gases:—

Gas.	Refractive Power.		Density.
	Index.	Compared with Air.	
Hydrogen	1·000138	0·470	0·069
Nitrogen	1·000300	1·020	0·971
Air	1·000294	1·000	1·000
Oxygen	1·000272	0·924	1·106
Chlorine	1·000772	2·623	2·470

And it will be seen that, with the exception of oxygen, this fulfils the condition mentioned above. But the position of air is most instructive: it is seen that, like its density, its refractive power is intermediate between that of its constituents. What stronger indirect evidence than this can we have that the velocity in a medium is due to the density of the molecules of that medium?

A further examination of this list is of value. We notice that oxygen is an exception. Now the other three gases are what chemists call monads; oxygen, on the contrary, is a dyad. The molecule of oxygen, let us assume, consists of one atom*, while the molecules of H, N, and Cl consist of two. Altering, then, the density of oxygen to one half that given, we find it occupies its proper place in the list. The following table exhibits this and other relations:—

Gas.	Density of Molecule.	Sq. Rt. of Density.	Sq. Rt. of Density. H=1.	Refractive Power. Air=1.	
				Calculated.	Observed.
Hydrogen ...	2	1·414	1	·470	·470
Oxygen	16	4·000	2·8	1·3160	·924
Nitrogen	28	5·2915	3·7	1·7390	1·020
Chlorine ...	71	8·426	5·9	2·7730	2·623

* Other considerations lead us to consider that the molecule of O consists of two atoms; if this be so, we must consider O an exception.

It is impossible not to be struck with the relation which is here exhibited, especially when we remember how many influences are at work interfering with the perfect freedom of motion which is necessary for this law to be rigorously true. Numerous confirmations of this molecular theory occur when we examine tables of the refractive indices of various bodies. We extract the following passages from the article "Light" in 'Watt's Dictionary of Chemistry,' vol. iii. pp. 616-618:—

"Generally speaking, the refractive power of *any one substance* increases with its density."

"The refracting power of liquids is diminished when they are expanded by heat."

Biot and Arago "found that at pressures not exceeding that of the atmosphere, the quantity $\mu^2 - 1$, which is called the absolute refractive power, is proportional to the density of the gas."

"Dulong has shown that the refractive power of a mixture of gases is equal to the mean of those of the constituent gases calculated for the pressure to which each gas is actually subjected in the mixture."

Compare the self-evident explanation offered of these results on the molecular theory with the complicated and unsatisfactory nature of that afforded by the ether theory, even after assuming the existence and all-pervading properties of this substance.

Again, in the complicated phenomena of interference and polarization, how few are the assumptions which we have to make! No difference in elasticity of a contained medium, owing to the different molecular groupings, but these different molecular groupings themselves all-sufficient to account for the phenomena.

Colour and chemical action are also found to be very much more easily explained, when we consider the molecules of bodies to vibrate instead of the molecules of the ether.

Summing these conclusions, we have:—

α . The molecular theory makes no departure in its assumptions from the analogies observed in nature.

β . The phenomena of refraction follow as a consequence of this theory.

γ . The complicated phenomena of colour, double refraction, polarization, and interference are all explained without making assumptions which have no analogies in observed facts.

δ . Independent phenomena, especially the increase of the refractive powers of gases with the increase of their densities, support this theory.

ϵ . The turning of the plane of polarization by the passage

of light through various substances placed in the magnetic field follows as a consequence of the influence exerted by the magnet on the molecules of the body.

In conclusion, in the following Table is drawn up a comparative view of the explanations and assumptions made in the two theories of the various phenomena of light.

Phenomena.	Ether Theory.	Molecular Theory.
Fundamental assumption.	Light is transmitted by the vibrations of an elastic and all-pervading medium.	Light is transmitted by the vibrations of the molecules of bodies.
Refraction ...	The elasticity or density of the ether is altered by its contiguity to the molecules of the refracting body.	The molecules of different bodies move with different degrees of freedom.
Colour	The ether particles vibrate with different degrees of velocity for different colours.	The molecules vibrate more rapidly for some colours than for others.
Calorescence and fluorescence.	The impact of the <i>etheral</i> waves causes the ether in the bodies to vibrate, sometimes with greater, sometimes with less velocity than the particles of ether in these waves.	The impact of the <i>molecular</i> waves causes the molecules of the bodies to vibrate.
Radiation and absorption...	The oscillation of the molecules of the radiating body throws the particles of ether in the surrounding medium into vibration. This vibration causes the ether in the absorbing body to vibrate; and the vibration of this ether causes the molecules of the absorbing body to vibrate.	The molecules of the radiating body vibrate; this throws the molecules of the surrounding medium into vibration; and these throw the molecules of the absorbing body into vibration.
Double refraction	Owing to the different grouping of the molecules of the crystal, the elasticity or density of the ether in which these molecules are contained is altered.	The freedom of the molecules to vibrate is different in different planes, owing to the molecular constitution of the crystal.
Chemical action	The vibration of the particles of the ether in which the molecules of the body are contained shakes these molecules, so as in one case to overcome the bond of chemical union between the atoms of the molecule, in the other case to cause the atoms to combine.	The vibration of the molecules of the body causes in one case the force of chemical affinity to be suspended; in the other, it causes the force to be brought into action.

XXXIX. *On the Modulus of Cohesion of Ice, and its bearing on the Theory of Glacial Erosion of Lake-Basins.* By R. D. OLDHAM*.

TAKING advantage of the late frost, I was able, through the kindness of Mr. Hall, the manager of the Victoria Lime and Cement Works, Rugby, to make a series of experiments with a view to determining the modulus of the cohesion of ice, so as to be able (reasoning from that) to determine whether it would be possible for a glacier to scoop out a lake-basin of any considerable size.

Before describing the results of these experiments, it may be well to mention the mode in which they were conducted. The machine used was an ordinary cement-testing machine, which, as used by me, was arranged as a simple lever of the second order multiplying five times. From the nature of the machine, it was impossible to arrange for a perfect counterpoise; nor, with the instruments at my disposal, could I make any accurate determination of the initial pressure on the test; but I was able to estimate it as not far from 20 lb., which I have in each case added to the pressure indicated, in order to obtain the full pressure on the test. The samples experimented on were frozen in cubical moulds of $1\frac{1}{2}$ in. in the side, and were for the most part perfectly clear and transparent, though just in the centre they were sometimes more or less opaque. As, however, this opaque portion was never more than $\frac{1}{2}$ inch in diameter, the error so introduced is insignificant; and as the specimens were exposed, in their frozen state, to temperatures never rising above freezing-point for periods varying from 4 or 5 to over 24 hours, there could be no interstitial moisture which would vitiate the results.

A few words in description of the behaviour of these samples of ice when subjected to increasing pressures may not be amiss. As the pressure was applied, the ice did not seem to yield at first; but as soon as the pressure reached about 150 lb. on the square inch, very evident signs of yielding showed themselves: first a crack would form in one part of the cube, the sides of which would slip over each other a little and then unite again; the same process would be repeated elsewhere, and then again somewhere else; so that, by a continuous giving way and reuniting, the ice would yield indefinitely to this pressure, though it would not actually crush. It is the pressure at this point which is noted in the Table below as the pressure at commencement of yielding—not because I believe that no

* Communicated by the Author.

pressure less than this could produce any change of shape in the ice (for I feel sure that, if continuous for an indefinite period, a pressure far less than this would be sufficient to produce an indefinite change of form), but because, from the nature of the case, any such slow and gradual yielding could not be detected, both on account of the warmth of the testing-room and the small range of motion allowed by the machine, while the pressure at which the ice began to yield by the continuous formation of small cracks could be comparatively easily determined with sufficient approach to accuracy.

As the pressure was increased the yielding went on faster and faster, till when a pressure of about 400 lb. on the square inch was reached the ice could no longer yield in this continuous manner, but was crushed to pieces. The pressure at this point is noted in the fourth and fifth columns of the Table below:—

Pressures.

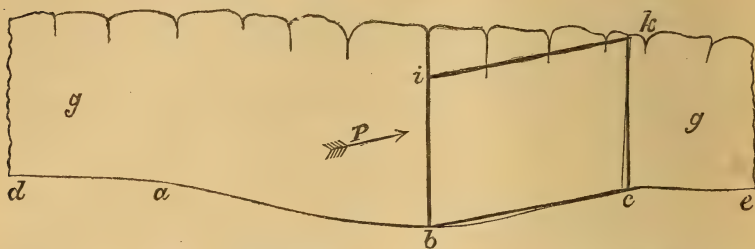
	For commencement of yielding.		For crushing.		
	Pounds per square inch.	Modulus, in feet.	Pounds per square inch.	Modulus, in feet.	
1.	354.5	869	} Not crushed.
2.	457.8	1122	
3.	369	904	
4.	121.5	297	
5.	153.4	376	
6.	164.5	403	457.8	1122	
7.	153.4	376	357.8	877	
8.	164.5	403	357.8	877	
Average ..	151.5	371	392.4	960	

The above Table shows that, under a pressure of a column of its own substance 370 feet in height, ice must yield, and that no slow pressure greater than one equivalent to that depth of ice could be transmitted by ice, while under a pressure of 960 feet of its own substance ice would be crushed; but, to prevent any error on the wrong side, I shall take the modulus of ice at 1000 feet, or nearly three times what was actually observed.

In order to apply these figures to the investigation of how far it would be possible for a glacier to scoop out a lake-basin of any considerable size, it will be necessary to form some idea of what is the friction between the base of a glacier and its bed. Now on this point we have, fortunately, experimental evidence; for the angle of repose of clean ice on moderately

rough sandstone has been determined by Mr. Hopkins* to be 20° . Now, as the base of a glacier has imbedded in its substance quantities of rock (for, if not, there could be no erosive power), the coefficient of friction must be greater than $\tan 20^\circ$ or $\cdot 577$. In the following calculations I have taken it at only $\cdot 2$, so that there shall be no chance of exaggeration†.

The next point to be determined is, what pressure acting parallel to the surface of the bed would be required to force a glacier *en masse* through and out of a lake-basin which it had filled in its onward course. In the figure let $g g$ represent a



glacier flowing over its bed $d a b c e$, and let $a b c$ represent the longitudinal section of a rock-basin which it has filled: it is required to determine the lowest pressure which will cause the prism $b c k i$ to be forced up the inclined plane $b c$. Then

$$P = w \sin \theta + \mu w \cos \theta,$$

where P is the pressure, expressed for convenience in vertical feet of ice, w the weight of the prism, μ the coefficient of resistance, and θ the angle of the slope $b c$. In this equation $w \sin \theta$ is constant; for w varies as $\text{cosec } \theta$, and is always equal to the resistance due to a column of ice in height equal to the depth of the lake-basin, which may be called D ; and since $w \sin \theta = D$, $\mu w \cos \theta$ will equal $\mu D \cot \theta$; so that the equation becomes

$$P = D + \mu D \cot \theta,$$

$$\text{or} \quad P = D(1 + \mu \cot \theta). \quad \dots \dots (I.)$$

Thus P increases approximately as $\cot \theta$.

* Quoted by the Rev. T. G. Bonney, M.A. (Quart. Journ. Geol. Soc. vol. xxvii. p. 322, 1871).

† Inasmuch as a glacier could not (if a rigid body) flow down an angle of less than 20° by its own weight, and as the upper surface of glaciers is known to move when the inclination is much less, it is evident that the resistance of ice to change of shape cannot be very great, or, in other words, that under a comparatively small pressure it apparently behaves as a viscous body. But this does not affect the question of whether the base slides over its bed: if the angle of slope be but 5° it could not, though that might be an angle sufficiently steep to allow the upper portions to slide over the lower.

It will be necessary also to determine what is the greatest pressure which could be transmitted by the prism $bck i$. This is governed partly by the modulus of cohesion of the ice, and also by the extra support given by the excess of thickness of the ice over b compared with that over c . Giving this its utmost power,

$$Q = m + x - y, \quad . \quad . \quad . \quad . \quad . \quad (II.)$$

where

Q = the ultimate strength of, or the maximum pressure that could be transmitted by, the prism $bck i$,

m = the modulus of cohesion,

x = the thickness over b ,

y = the thickness over c .

But it is evident that if P is greater than Q , or, in other words, if the pressure required to overcome the resistance is greater than the utmost pressure that can be transmitted, no motion as a whole can ensue, and consequently no abrasion of the bed bc can take place; so that for given values of θ , x , y , and μ , D attains its maximum when

$$P = Q,$$

or

$$m + x - y = D(I + \mu \cot \theta),$$

or

$$D = \frac{m + x - y}{1 + \mu \cot \theta} \cdot . \quad . \quad . \quad . \quad . \quad . \quad (III.)$$

In the above equations the pressure has been supposed to be limited, in the first case, only by the resistance to be overcome, and in the second by the ultimate strength of the ice. But if the slope of the bed of the lake-basin from a to b is less than the angle of repose—that is (taking β as the angle of slope), if $\tan \beta$ is less than μ , the ice cannot slide down of its own weight, but must be pushed down; and consequently the actual pressure exerted at b could not equal Q , but would be diminished by a quantity equal to that due to the resistance to motion down ab . Here the same equations as before hold good, substituting $-\beta$ for θ ,

$$S = w \sin -\beta + \mu w \cos -\beta;$$

or

$$S = \mu w \cos \beta - w \sin \beta;$$

or

$$S = D(\mu \cot \beta - 1). \quad . \quad . \quad . \quad . \quad . \quad (IV.)$$

But the total pressure which could be transmitted at a only

equals m ; so that the total depth to which a glacier could be forced down a slope is

$$D = \frac{m}{\mu \cot \beta - 1}.$$

Or if p represent the thickness of the glacier at the head of the slope, and r that at the lowest point to which it can be forced,

$$D = \frac{m + p - r}{\mu \cot \beta - 1}. \quad \dots \quad (V.)$$

Suppose T to be the total pressure which, if applied at a , would be required to impel the glacier, as a whole, through and out of the basin abc ; then

$$\begin{aligned} T &= P + S \\ &= D(1 + \mu \cot \theta) + D(\mu \cot \beta - 1) \\ &= \mu D (\cot \theta + \cot \beta). \quad \dots \quad (VI.) \end{aligned}$$

But as the pressure which can be transmitted at a is only m , we get the greatest value for D when

$$m = \mu D (\cot \theta + \cot \beta),$$

or

$$D = \frac{m}{\mu (\cot \theta + \cot \beta)},$$

supposing the thickness of the glacier at the lower end to be the same as that at the head of the lake; but if there be any difference, D cannot be greater than

$$D = \frac{m + z - y}{\mu (\cot \theta + \cot \beta)}, \quad \dots \quad (VII.)$$

z being the thickness at the upper, and y at the lower end of the lake-basin.

As, however, it will not always be necessary or convenient to find the maximum value of D , but rather of ab (that is, the maximum distance to which motion could be transmitted through a glacier as a whole), this may easily be deduced from the above formulæ; for if L represent the extreme distance to which motion can be transmitted, then $D = L \sin \theta$ or $L \sin \beta$, as the case may be. Substituting this in (III.) and (V.),

$$L = \frac{m + x - y}{\sin \theta + \mu \cos \theta}, \quad \dots \quad (VIII.)$$

or

$$L = \frac{m + p - r}{\mu \cos \beta - \sin \beta}; \quad \dots \quad (IX.)$$

both of which, when the slope vanishes, become

$$L = \frac{m+i-l}{\mu}, \dots \dots \dots (X.)$$

i being the thickness at the commencement of the level ground, and l at the extreme point to which motion could be communicated.

I would here call special attention to one point proved by the above formulæ, and which is at first sight totally opposed to the idea one might naturally form of what was actually the case,—namely, that the resistance which would be opposed to a glacier moving *as a whole* through any depression that might lie in its path is shown (by formula V.) to *increase* as the slope leading out of that depression diminishes, and approximately in the ratio of the cotangent of the angle of slope: thus, for an angle of $1'$ the resistance would be ten times that for an angle of $10'$, for an angle of $10'$ about ten times that due to a slope of $1^\circ 40'$, and for a slope of 5° about one fifth that due to a slope of 1° . This is of the greatest importance; for wherever the theory of glacial erosion is upheld, especial emphasis is laid on the fact that the hollows in which lakes are situated are of but insignificant depth compared with their length—a fact which investigation shows to be the very point that would make the excavation of lake-basins of any great size by these means not only improbable but absolutely impossible.

But as formulæ, in the state of formulæ, are distasteful and unintelligible to many, I will apply the formulæ deduced above to actual examples; and for this purpose I shall begin by considering the case of the Lake of Geneva, as it is the one concerning which I can obtain the most perfect data, and which, through the wide-spread circulation of Professor Ramsay's 'Physical Geography and Geology of Great Britain,' and from the fact that it was selected as an illustration in his original memoir, is best known. From Professor Ramsay I take the following data:—extreme length 45 miles; extreme depth 984 feet, say 1000 feet (for the lake must have been at least that depth originally); distance of greatest depth from lower end 25 miles,—giving a slope into the lake of $33'$ and out of the lake as $26'$, taking both as uniform.

Now the pressure necessary to force the glacier *en masse* through the lake is, by (VI.),

$$T = \mu D (\cot \theta + \cot \beta).$$

Here $D=1000$, μ is taken at $\cdot 2$, $\cot \theta = 132\cdot 22$, and $\cot \beta = 104\cdot 17$; whence

$$T = 47,278;$$

so that it would require a pressure of over 9 miles of ice to force a glacier *en masse* through and out of the Lake of Geneva. Compare this with the observed modulus, and further comment is superfluous.

Having thus proved that, in the case of the Lake of Geneva, the theory of glacial erosion is inadmissible, it may be well to show what is the very largest lake-basin that could possibly be scooped out by a glacier. To this end let both θ and $\beta = 5^\circ$, a supposition more favourable than is found to be the case in nature; then by (VII.),

$$D = \frac{m + z - y}{2\mu \cot 5^\circ}.$$

Here, taking y as $\frac{1}{2}z$ and μ as $\cdot 2$, we get the result that a glacier, 5000 feet in depth at the head of a lake-basin and thinning off to 2500 feet at its base, *could* not scoop out a lake of more than 700 feet in depth under any circumstances whatever, nor indeed could it scoop out one of even that depth; but I am at present only attempting to find a limit to its power.

One more point. The greatest distance to which a glacier could be forced *en masse* is given by (X.) as

$$L = \frac{m + i - l}{\mu}.$$

Here, taking i as 5000 feet and l as nothing, we get $L = 30,000$ feet, or rather over 5 miles; so that a glacier debouching on a plain could not exert any erosive power on that plain for more than five miles from the commencement of its level course, and consequently could not scoop out a lake-basin of more than that length, whatever its depth might be, nor could it be pushed over a plain *en masse* for more than that distance; but if it did extend further, this could only be possible by the sliding of the upper over the lower portions of the glacier, by virtue of the pseudo-fluidity of ice.

These last figures also show the fallacy of the idea that a vast ice-cap*, such as is supposed by some to have covered the greater part of Scotland, Ireland, and Wales, and even to have extended continuously to Scandinavia, could move *en masse* over distances measured, not by miles, but by hundreds of miles, passing in its onward career over hill and valley, mountain and plain, with one general movement of its own, totally independent of the shape of the ground over which it moved, and everywhere polishing and scratching the rocks over which it passed in one general direction—that of its own

* I owe this suggestion to Mr. R. Mallet, F.R.S.

motion. Such an ice-cap may have existed, and under favourable circumstances may have had some motion ; but if so, the motion was confined to the upper layers, for the lower portion *must* have been landlocked and stagnant.

This note has already run to some length ; so I shall conclude by pointing out:—that the figures given above are not meant to represent what a glacier actually can do, but the very outside limit of what it could possibly do, and, as such, a very large discount may be taken from them without altering their truth ; that they will hold good on any theory of glacier-motion, for the resistances of friction and gravitation will remain ; and that the points I claim to have proved are as follows:—

1. That no lake-basin exceeding 700 feet in depth or 5 miles in length could possibly owe its origin to glacial erosion, though the true limits are probably not one tenth of these quantities.

2. That no glacier could be pushed *en masse* over a plain for more than 5 miles.

3. That, consequently, no ice-cap could travel *en masse* over large areas independently of the conformation of the ground over which it travelled.

January 25, 1879.

PS.—Since writing the above, it has been pointed out to me that it is hardly justifiable to apply the formulæ of rigid dynamics to the case of a body like ice, which, when moving in large masses, assumes many of the appearances of a liquid in motion. I may therefore point out that the idea in the above paper is that, as soon as the resistance offered by friction rises to that point at which it would prevent all motion in the glacier if the glacier behaved as a perfectly rigid body, that point gives a limit beyond which it would not be possible for any motion of the base of the glacier over its bed to take place.

Mr. Robert Mallet authorizes me to state that, according to experiments made by Professor Phillips, the modulus of ice is reduced to almost nothing by the presence of interstitial moisture ; so that if, as is generally supposed to be the case, glacier-ice is permeated by interstitial moisture, the power of a glacier in scooping out lake-basins would be reduced to almost nothing.

Rugby, Feb. 15, 1879.

XL. On the Luminosity of Gases through Electrical Discharges. Supplement to the Paper on the Nature of Spectra.
By EILHARD WIEDEMANN*.

IN an investigation published in this Journal† I expressed the opinion that by the electric spark, independently of the temperature of a mixture of gases, certain particles are rendered luminous, and that the luminosity is not a direct consequence of a great rise of temperature, like the brightness of incandescent solids, for instance, or that of sodium vapour in a gas-flame. I have, on that account, compared the phenomenon in question to the phenomena of fluorescence.

New experiments have confirmed this view, and yielded the result, *that a gas may become luminous, on electricity passing through it, while yet its temperature is far below 100°.*

For the experiments a discharge-tube was used consisting, first, of a wider portion 30 millims. in diameter and 90 millims. in length, which was conically drawn out at its ends. To one end a glass bulb provided with a glass cock was fused, in the middle of which was an aluminium knob, serving as electrode; to the other end a capillary tube bent in the shape of a U, diameter 0·854 millim., height of the U about 93 millims., was joined by fusion, to which was attached a glass bulb with cock and aluminium electrode.

The U-tube was placed in a calorimeter, which consisted of a brass tube filled with oil of turpentine into which a thermometer dipped, and was enclosed in a double-walled vessel filled with water. The waterworth of the entire calorimeter inclusive of the oil of turpentine, the thermometer, and the immersed portion of the U-tube, amounted in the experiments to 8·846 grams.

The discharge-tube was filled with air and exhausted (to about 3 millims.) till the entire wide tube was completely filled with continuous light when the discharges passed through it. A very feeble stratification appeared momentarily in isolated cases only. After the pressure was read off, the cock communicating with the air-pump was closed.

A Ruhmkorff induction-coil of medium size, with a mercury interruptor, served as the source of electricity. By a simple arrangement a black-writer was inserted, through a relais, in a second current-circuit, which marked the number of the closings of the Ruhmkorff, and therefore also the number of the discharges through the Geissler tube.

* Translated from a separate impression, communicated by the Author, from Wiedemann's *Annalen*, vi. pp. 298-302.

† Phil. Mag. Feb. 1879, pp. 77-95.

The inductorium was excited by two Bunsen elements. The calorimetric determinations were carried out in the following manner:—First, for 5–10 minutes before the experiment the course of the thermometer was observed; then, exactly at the minute, the primary circuit of the inductorium was closed. When with the play of the interruptor a sufficient augmentation of the temperature of the calorimeter had taken place, the current-circuit was again opened, at a moment which was read off, and the course of the thermometer followed again during 5–10 minutes. From the rise of temperature which had taken place, corrected in the well-known manner, and from the number of the discharges, counted on the strip of paper of the marker, the quantity of heat generated at each single discharge in the capillary tube could be calculated. From this, and from the dimensions of the capillary tube, the temperature of the gas can be approximately ascertained, provided that its specific heat does not alter much with the temperature.

From the older experiments of G. Wiedemann and the newer ones of A. Naccari and Bellati, however, it follows that the quantity of heat generated in each cross section of a discharge-tube at the passing of the discharge is independent of the magnitude of the section, and in very wide tubes is somewhat less. It thence follows, further, that the increments of temperature must be inversely proportional to the cross section, and so the increments of temperature of the gas in the wider tube can be calculated from those observed in the narrower one.

If p is the pressure of the gas, V the volume of the heated gas in the capillary tube below the surface of the oil of turpentine, z the number of discharges in a minute, Z the time, in minutes, during which the discharges pass through the gas, t the corrected increase of temperature generated in the calorimeter, c the specific heat of the gas, s its specific gravity at 0° , w the waterworth of the calorimeter, then the increment of temperature T of the gas, to be calculated from the above quantities, at each discharge in the capillary tube is very nearly

$$T = \frac{wt \cdot 760}{VscpZz}^*.$$

* For c I have introduced the specific heat at a constant pressure, 0.237. It might, however, be possible for the heating in a part, at least, of the capillary tube to take place at constant volume; the numbers found for T and τ would then become $\frac{1}{4}$ higher. In face of this uncertainty the error vanishes completely which we have committed in putting for s the specific gravity at 0° and not that at the temperature of the experiment.

Dividing T by the ratio of the cross section of the wider tube to that of the narrower (1232), we get the increment of temperature in the former,

$$\tau = \frac{T}{1232}.$$

Of a whole series of experiments I give the following five, together with the values obtained from them for τ and T:—

<i>p.</i>	V. cub. cent.	<i>z.</i>	Z.	<i>z.</i>	T.	τ .
3.3	0.09774	350	9	3.93	84340	68.0
3.3	"	342	8	3.24	80130	65.2
3.3	"	348	8	3.52	85560	69.5
2.66	"	348	10	3.51	86360	70.4
2.66	"	338	14	4.35	77250	62.0

As the mean temperature of the gas, before the passing of the discharge, amounted to about 20°, the maximum temperature generated in the first tube was about 80–90°; and therefore the gas was brightly luminous.

It is necessary to remark that if alternating partial discharges take place, the above temperature is still too high; and so it is when the discharges do not take place momentarily, but last a certain time. Further, since the gas constantly becomes quite dark again between every two discharges, and shines as brightly after the first as after the later ones, the luminous appearances cannot be conditioned by heatings being accumulated by the successive discharges. The temperature 62–70° is at all events not the lowest at which the gas appears luminous; for when the discharges of a Holtz machine were conducted through a discharge-tube precisely similar to that above described, the wider part of it appeared completely filled with light, while the light in the narrower part was much fainter than when the inductorium was employed. Exact calorimetric measurements are, in consequence of the inconsiderable production of heat, much more difficult to make with the former than with the latter.

The luminousness of the gas at so low a temperature during the passage of electricity proves, when viewed in connexion with the mechanical theory of gases, that the electric discharge, independently of an augmentation of the *vis viva* of the progressive motion of the molecules by temperature, calls forth a considerable heightening of the *vis viva* of the oscillatory motion of the æther envelopes.

To make use of the result here found for explaining what goes on in the discharges in gases, and the nature of the elec-

tricity-motion, as well as for the application of electrical discharges to the study of the spectra of gases, is reserved for separate investigations. Meanwhile so much is even now evident, that the different spectra in the parts of different width of discharge-tubes are not to be referred alone to the different temperatures of the gases, but depend essentially upon the amounts of electricity the passage of which conditions the oscillatory motions of the æther envelopes of every individual atom or molecule.

By means of such calorimetric measurements we might indeed succeed in determining the quantities of heat which are necessary in order so to alter the state of the molecules and atoms that the band spectrum shall change into the line spectrum; or, in other words, if we adopt the views developed in the former paper, we must be able to ascertain the amount of heat which is set free at the formation of the molecule of a simple body out of its atoms.

I shall shortly communicate something further upon these subjects.

Leipzig, January 1879.

XLI. *On the Determination of the Variation of the Thermal Conductivity of Metals with Temperature, by means of the permanent Curve of Temperature along a uniform thin Rod heated at one end.* By OLIVER J. LODGE, D.Sc., Lecturer on Applied Mathematics and Mechanics at University College, London.

[Concluded from p. 211.]

Introduction of the experimental Values of the Variables into Equation (3) and the first Integration of it.

16. **W**HAT we have accomplished so far is:—the writing of the fundamental equation (1) by help of equation (2) in the form (3), which involves the ratio of rate of cooling θ to thermometric conductivity $\frac{k}{cp}$; and then the expression of these two quantities as functions of the temperature—the one as a complex function (5), the other as an inverse linear function (4). The latter contains Centigrade temperature t ; but if we reckon temperature from the temperature of the enclosure v_0 instead of from the Centigrade zero, it will only affect the value of the constant b . So writing $v_0 + b - 274 = m$, we get (4) in the form

$$\frac{k}{cp} = \frac{A}{m + \theta} = \frac{mk_0}{c_0\rho_0(m + \theta)}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (9)$$

Hence the equation (3) may now be written

$$\frac{d^2\theta}{dx^2} = \frac{m+\theta}{A} \{Pa^{v_0}(a^\theta-1) + D\varpi^g\theta^{1.232}\}, \quad . \quad . \quad (10)$$

or, say, for shortness,

$$\frac{d^2\theta}{dx^2} = \{R(a^\theta-1) + S\theta^{1+s}\}(m+\theta). \quad . \quad . \quad (11)$$

This equation has now to be integrated. Whether it can be integrated completely as it stands I do not know; but a first integration is easy, and the result is

$$\begin{aligned} \frac{1}{2}\left(\frac{d\theta}{dx}\right)^2 &= \frac{R(a^\theta-1)}{\log a} \left(m+\theta - \frac{1}{\log a}\right) - R\theta \left(m - \frac{1}{\log a}\right) - \frac{1}{2}R\theta^2 \\ &\quad + S\theta^{2+s} \left(\frac{m}{2+s} + \frac{\theta}{3+s}\right). \quad . \quad . \quad . \quad . \quad . \quad (12) \end{aligned}$$

17. In this expression the integration has been performed between the limits 0 and θ ; or one may say that the arbitrary constant has been chosen so as to make $\frac{d\theta}{dx}$ and θ vanish together—which it is evident they do in an infinitely long rod, from physical considerations. This is the object of using a long rod. The experiment would be easier to carry out with a short rod or a ring heated at one end and cooled at the other; and the differential equation (1) would apply equally well; but in this case $\frac{d\theta}{dx}$ would have a finite value when $\theta=0$, the right-hand side of (12) would not contain θ as a factor, and the whole calculation would become more complicated.

Statical Curve of Temperature in a Vacuum.

18. Equation (12) consists of two parts—the R part relating to radiation, the S part to convection: in a vacuum S is zero. In order to do any more with the equation, I must take the two parts separately. The radiation part is the simpler. So let us suppose the rod on which the curve of temperature is being observed has a blackened surface, and is in a perfectly exhausted enclosure. The convection part I have only slightly attacked at present, and have not succeeded in doing any thing practical with it. I am unable to integrate even the radiation part of (12) any further as it stands; so at this stage we will introduce our very approximate expansion (7') for $a^\theta-1$; and

we get the radiation part of (12) in the form

$$\frac{1}{2} \left(\frac{d\theta}{dx} \right)^2 = R\theta \left\{ 1 + \frac{1}{2} \alpha \theta + \frac{1}{6} \alpha^2 \theta^2 (1 + \gamma) \right\} \left(m + \theta - \frac{1}{\alpha} \right) - R\theta \left(m - \frac{1}{\alpha} \right) - \frac{1}{2} R\theta^2,$$

where α means $\log \alpha$, and where γ is written for the correction factor $\frac{1}{8} \alpha \Theta$, or $\frac{\Theta}{1000}$. And this equation simplifies to

$$\left(\frac{d\theta}{dx} \right)^2 = R\alpha \theta^2 \left\{ m + \frac{\theta}{3} (2 + m\alpha + \gamma(m\alpha - 1)) + \frac{\alpha \theta^2}{3} (1 + \gamma) \right\}. \quad (13)$$

Although, therefore, the equation looked as though it would contain, when put into the ordinary form for integration, a quartic expression under the root, and therefore would land us in elliptic functions, yet on working it out we find that θ^2 is a factor of the expression; and hence, the expression under the root being only of the second degree, the integration can be performed without difficulty. Integrating between the limits 0 and x , and between Θ and θ , and remembering that $\frac{d\theta}{dx}$ is essentially negative, we get as the result,

$$\left[\sinh^{-1} \frac{\frac{6m}{\theta} + 2 + m\alpha + \gamma(m\alpha - 1)}{\sqrt{12m\alpha(1 + \gamma) - \{2 + m\alpha + \gamma(m\alpha - 1)\}^2}} \right]_{\Theta}^{\theta} = x \sqrt{Rm\alpha}. \quad (14)$$

or

$$\sinh^{-1} \left(\frac{L}{\theta} + J \right) = \sinh^{-1} \left(\frac{L}{\Theta} + J \right) + x \sqrt{Rm\alpha}; \quad . \quad . \quad (15)$$

where L and J are used as abbreviations for certain evident expressions such that the ratio of J to L , which is all we need trouble about, is

$$\frac{J}{L} = \frac{2 - \gamma + m\alpha(1 + \gamma)}{6m} = \lambda \text{ say}. \quad . \quad . \quad (16)$$

It is quite possible, however, for L and J to be both imaginary, which happens when the second term under the root on the left-hand side of (14) is greater than the first. When this is the case I suppose the \sinh becomes \cosh , passing into it through infinity; but the ratio $J : L$, or λ , is always real and positive.

Referring back to § 7 and to equation (9), we see that a likely value of m for iron is 300, and for copper 650; so that for these two metals the value of $m\alpha$ is about 2.3 and 4.9 respectively. Looking at the expression under the square root on the left-hand side of (14), and neglecting squares of the small quantity γ , one

sees that $m\alpha$ may vary between $4+5\gamma \pm 2\sqrt{3(1+2\gamma)}$ without the change from \sinh to \cosh taking place; hence the above form is likely to cover the case of all ordinary metals.

19. We will denote the constant $\frac{L}{\Theta} + J$ by the letter K , and the constant $\sqrt{Rm\alpha}$ by the letter μ , and will then write the equation (15) in the form

$$\frac{L}{\Theta} + J = K \cosh \mu x + \sqrt{K^2 + 1} \sinh \mu x; \quad \dots \quad (17)$$

or, what is equivalent,

$$\frac{L}{\Theta} + J = \frac{1}{2}(\sqrt{K^2 + 1} + K)e^{\mu x} - \frac{1}{2}(\sqrt{K^2 + 1} - K)e^{-\mu x}. \quad (18)$$

20. Now the minimum possible value of L is $\frac{\sqrt{3m}}{\sqrt{1+5\gamma}}$ (this being its approximate value when $m\alpha = 4+5\gamma$); and as m is a number likely to be bigger than Θ , and as J is always positive and mostly greater than 1, it follows that K , or $\frac{L}{\Theta} + J$, is seldom small; and a reasonable value for it is about 5; hence K and $\sqrt{1+K^2}$ are ordinarily not very unequal. For a case when they may be regarded as practically equal the second term of equation (18) vanishes; and it may be written, on this assumption,

$$\frac{L}{\Theta} + J \simeq Ke^{\mu x}, \quad \dots \quad (19)$$

or, putting in the value of K , namely $\frac{L}{\Theta} + J$, and writing $\frac{J}{L} = \lambda$ (see equation 16),

$$\Theta \simeq \frac{\Theta e^{-\mu x}}{1 + \lambda \Theta (1 - e^{-\mu x})}. \quad \dots \quad (20)$$

Amount of Divergence of the corrected Curve from the Logarithmic Form.

21. As the result, then, of the whole investigation, we have the two equivalent equations (17) and (18), and the approximately equivalent equation (20); which last, however, is only true when K is so large that there is no perceptible difference between K^2 and $1+K^2$. It remains to see how far these equations agree with the results of the experiments made hitherto, and to show how the conductivity k and the variation-coefficient of conductivity m can be deduced from them.

Now equation (20) obviously reduces to the ordinary logarithmic curve, which was supposed by Biot to represent the curve of temperature along the rod, by making $\lambda = 0$. Equa-

tion (14) also reduces to the same curve $\theta = \Theta e^{-\mu x}$, if we make m infinite and α and γ both zero, *i. e.* if we neglect variations in conductivity and radiation-power; for the numerator is then $\frac{6m}{\theta}$, and the denominator is indeterminate indeed, but finite.

My brother, Mr. Alfred Lodge, of St. John's College, Oxford (to whom I am indebted for several suggestions) has drawn for me the curve represented by equation (20) for some arbitrary and rather extreme values of the constants, *viz.*

$\lambda = \frac{1}{m}$, say $= \frac{1}{300}$, $\Theta = 300$, and $\mu = \frac{1}{80}$; that is, the curve

$$\theta = \frac{300 e^{-\frac{x}{80}}}{2 - e^{-\frac{x}{80}}};$$

and it is represented in Plate X., where, for comparison, is drawn also the logarithmic curve

$$\theta = 300 e^{-\frac{x}{80}},$$

and also (by simply diminishing all the abscissæ in the ratio 3 : 2) the logarithmic curve which fits the correct curve best, and which would have been assumed to be the correct one from calculation of μ from observed values of the temperature by the ordinary formula, namely the curve

$$\theta = 300 e^{-\frac{x}{53}}.$$

The value of μ is therefore not obtained correctly, at least as regards its absolute value, by the old process; and the nature of the divergence between the new and the old curves is apparent in the Plate.

The Method of Calculating the Conductivity and its Variation-coefficient by means of Equation (17) to the Curve of Temperature.

22. The conductivity $\frac{k}{c\rho}$ is involved in the constant μ ; so that if one knows μ , the absolute conductivity can be calculated. For the meaning of μ refer to equations (17), (15), (11), (10), (9), (6), and (7), which show that it may be written in the following equivalent ways:

$$\mu^2 = Rm\alpha = \frac{m}{A} P a^{\gamma_0} \log a = \frac{c_0 \bar{\rho}_0}{k_0} \cdot P a^{\gamma_0} \log a = \frac{2c \cdot \rho \cdot C}{k_0 \log a} \quad . \quad (21)$$

Hence μ is inversely proportional to the square root of the conductivity; and if the values of μ are known for any two metals coated with the same varnish and otherwise under precisely similar conditions, their thermometric conductivities at zero Centigrade are inversely as μ^2 ; so that

$$\frac{k_0}{k'_0} = \frac{c_0 \rho_0}{c'_0 \rho'_0} \cdot \frac{\mu'^2}{\mu^2} \quad \dots \quad (22)$$

Or, again, the conductivity of any one metal may be expressed in absolute measure as soon as we know μ and have determined the radiation-constant P (or C) by direct experiment on the rate of cooling of the rod *in vacuo*; for

$$\frac{k_0}{c_0 \rho_0} = \frac{P a^{v_0} \log a}{\mu^2} \quad \dots \quad (23)$$

The mode of calculating P is given in § 27 and equation (32).

On the other hand, the variation-coefficient m is involved in the constant λ ; so that, if one knows λ , it can be easily calculated. For the meaning of λ , refer to equations (16), (13), and (7'), which show that its value is

$$\lambda = \frac{1}{3m} + \frac{\log a}{6} + \frac{\Theta}{6000} \left(\log a - \frac{1}{m} \right);$$

or, expressing m in terms of λ ,

$$m = \frac{2 - \gamma}{6\lambda - \alpha - \alpha\gamma} = \frac{2000 - \Theta}{6000\lambda - 7.6 - .0076\Theta}; \quad (24)$$

and as soon as m is known, the law of variation of conductivity with temperature can be expressed by the equation

$$\frac{k}{c\rho} = \frac{A}{m + \theta} = \frac{A}{b + t} \quad \dots \quad (9)$$

where $b = m + 274 - v_0$; compare equations (9) and (4).

Hence what we have to do is to determine the constants λ and μ from observed values of the temperature down the rod, on the hypothesis that this curve of temperature is correctly represented by equation (18).

Calculation of the Constants λ and μ .

23. Let the excess of temperature of the rod over the enclosure be observed very accurately (by thermoelectric or other means) at equal intervals all along the rod, say at successive distances from the origin ξ , 2ξ , 3ξ , &c. Call any three consecutive values of these temperatures θ_1 , θ_2 , and θ_3 ; then it is easy to show from equation (18) that the quotient

$$\left(\frac{L}{\theta_1} + J + \frac{L}{\theta_3} + J \right) \div \left(\frac{L}{\theta_2} + J \right)$$

is constant all down the rod, and equal to $e^{\mu\xi} + e^{-\mu\xi}$, or that

$$\frac{\frac{1}{\theta_1} + \frac{1}{\theta_3} + 2\lambda}{\frac{1}{\theta_2} + \lambda} = \text{const}^* = 2r \text{ say,} \quad \dots \quad (25)$$

and that the value of the constant r is

$$r = \cosh \mu\xi. \quad \dots \quad (26)$$

Hence to find μ we must know r , and to find r we must know λ .

24. Now the only way to determine λ accurately is by a system of trial and error, choosing it so that the expression (25) shall be as constant as possible all down the rod; but a good approximation to its value can be obtained thus. Let $\theta_1, \theta_2, \theta_3$ be three equidistant temperatures near the hot end of the rod, and $\theta_4, \theta_5, \theta_6$ three equidistant temperatures near the cool end; then of course

$$\frac{\frac{1}{\theta_1} + \frac{1}{\theta_3} + 2\lambda}{\frac{1}{\theta_2} + \lambda} = \frac{\frac{1}{\theta_4} + \frac{1}{\theta_6} + 2\lambda}{\frac{1}{\theta_5} + \lambda};$$

and therefore

$$\lambda = \frac{\left(\frac{1}{\theta_1} + \frac{1}{\theta_3}\right)\frac{1}{\theta_5} - \frac{1}{\theta_2}\left(\frac{1}{\theta_4} + \frac{1}{\theta_6}\right)}{\frac{2}{\theta_2} + \frac{1}{\theta_4} + \frac{1}{\theta_6} - \frac{1}{\theta_1} - \frac{1}{\theta_3} - \frac{2}{\theta_5}}, \quad \dots \quad (27)$$

an expression which is rather long, but with the aid of a table of reciprocals can be evaluated without difficulty. The value of λ so obtained may be introduced into (25) and improved by successive approximations; after which it is to be introduced into (24) and the number m obtained, which, when inserted in equation (9), expresses the rate of variation of conductivity with temperature for the metal experimented upon.

Calculation of the Constant μ .

25. The above process, however, not only determines λ , but also gives us a number of values for r ; and from the mean of these we must proceed to obtain μ .

* This equation becomes identical with the one hitherto used, viz. $\frac{\theta_1 + \theta_3}{\theta_2} = 2 \cosh \mu\xi$, as soon as one puts $\lambda = 0$, and assumes θ_2 to be a geometric mean between θ_1 and θ_3 .

To find μ from (26), we may either write it thus,

$$\mu\xi = \cosh^{-1} r = \log_e (r + \sqrt{r^2 - 1}), \quad . \quad . \quad (26')$$

or, what is usually simpler in practice, we may notice that for the majority of metals r is but a fraction over unity, and hence that the number $\mu\xi$, whose hyperbolic cosine is equal to r , must itself be very small, and high powers in its expansion may be neglected with impunity. Writing (26) therefore thus,

$$r = \cosh \mu\xi = 1 + \frac{\mu^2 \xi^2}{2} + \frac{\mu^4 \xi^4}{24} + \frac{\mu^6 \xi^6}{720} + \dots,$$

the last term written is nearly always utterly negligible, and the last but one is usually exceedingly small. Hence a first approximation to μ is

$$\mu^2 \xi^2 \doteq 2(r-1); \quad . \quad . \quad . \quad (28)$$

and a second and generally sufficient approximation is

$$\mu^2 \xi^2 \doteq 2(\sqrt{3+6r}-3); \quad . \quad . \quad . \quad (29)$$

and as ξ , the distance between successive thermometers, is known, μ is determined.

Expression for the Relative Conductivities of two Metals in terms of the Constant r.

26. Equation (22), for the relative conductivities of two metals under precisely similar circumstances, becomes, therefore, if the intervals ξ are the same for both,

$$\frac{k_0}{k'_0} = \frac{c_0 \rho_0}{c'_0 \rho'_0} \left\{ \frac{\log (r' + \sqrt{r'^2 - 1})}{\log (r + \sqrt{r^2 - 1})} \right\}^2, \quad . \quad . \quad (30)$$

or, to an approximation sufficient for all but very badly conducting metals like bismuth,

$$\frac{k_0}{k'_0} \doteq \frac{c_0 \rho_0}{c'_0 \rho'_0} \cdot \frac{\sqrt{(3+6r')} - 3}{\sqrt{(3+6r)} - 3} \doteq \frac{c_0 \rho_0}{c'_0 \rho'_0} \cdot \frac{r' - 1}{r - 1}, \quad . \quad (31)$$

where it will be remembered that c_0 and ρ_0 mean respectively the specific heat and the density of the metal at zero Centigrade.

To determine the *absolute* conductivity, we must determine the constant P by direct experiments on cooling (see equation 23).

On the Determination of the Radiation-Constant P.

27. The rod, or a bit of the rod, is to be heated, as a whole, to some moderately high temperature, and then placed in the exhausted receiver under precisely the same external conditions as it is exposed to during the conduction experiments, its

two ends being rendered as impervious to heat as possible by means of a thickness of felt. Observations of its excess of temperature are then to be taken at successive intervals of time. If the excess of temperature θ_0 corresponds to the era of reckoning time, and if the temperature θ be the excess after a lapse of time τ , it follows, since

$$-\frac{d\theta}{d\tau} = Pa^{v_0}(a^\theta - 1)$$

(see equation (5) and § 3), that

$$\log \frac{a^\theta}{a^\theta - 1} = \log \frac{a^{\theta_0}}{a^{\theta_0} - 1} + Pa^{v_0}\tau \log a;$$

whence we obtain the constant required,

$$\begin{aligned} Pa^{v_0} &= \frac{1}{\tau} \log_a \frac{1 - a^{-\theta_0}}{1 - a^{-\theta}} \\ &= \frac{\log_{10} a}{\tau} \log_{10} \frac{1 - a^{-\theta_0}}{1 - a^{-\theta}}. \quad . \quad . \quad . \quad (32) \end{aligned}$$

The Absence of Experimental Results suitable for applying the method of calculation to at present.

28. To obtain experimental results suitable for applying equation (25), one would have to observe temperatures very accurately, over a considerable range of temperature, on a rod *in vacuo* with a highly radiating surface. Principal Forbes's experiments were all made in air; and, as he shows, convection had a greater cooling effect than radiation, even on his paper-covered rod. The only experiments which, so far as I know, have been made in a vacuum, are those of Wiedemann and Franz; but the range of temperature observed by them only went as high as 60° C. Moreover the surface of their rods was *silvered*, and the loss of heat therefore so small that they themselves did not regard these experiments as at all so satisfactory as those which they made in air.

Moreover the rods used were not very long, and their cool ends were connected up with the enclosure, which would doubtless make $\theta = 0$ at that end, but it would not secure that $\frac{d\theta}{dx}$ should vanish at the same time: and this was of no consequence in their method of calculation; but it is in ours (see § 17).

Again, the temperatures, though very carefully observed, cannot, I imagine, be regarded as accurate even to the first decimal place. I have applied equation (27) to their numbers

for iron, after reducing their galvanometer-deflections to Centigrade degrees by means of their little interpolation table; but it appears impossible to get any value for λ . The form given is very nearly $\frac{0}{0}$.

A simple modification of their method of observing temperatures, devised by Professor G. C. Foster, seems, however, to promise very accurate results; and it was in view of being thus able to observe temperatures with great accuracy that I set to work at the preceding calculation.

Suggestions for future Experiment.

29. In any experiments which may be conducted *in vacuo* there will probably be some difficulty with the radiating surface of the rods, which should be the same for all. Silvering has only one objection; but that is fatal—viz. that it diminishes the radiation-constant so much that the heat flows down the rod almost as if it were a slab; moreover the convection effect of residual air cannot be neglected in comparison with the radiation with so great impunity as if the surface were lampblackened. A coating of Brunswick black would probably be perpetually spoiling the vacuum. A close layer of thin paper (after Forbes) seems a very good method, or perhaps a close spiral of very thin cotton thread wound over the rod; but it cannot be said that the law of cooling in such cases is so well known as it is for a metallic or blackened surface. Probably a coating of stove-blacklead would be the best.

Any such covering would, of course, necessitate fixed thermoelectric joints, say very fine wires passed into or through the rod at certain accurately measured and equal intervals. The electromotive force generated at each joint would be measured directly by a compensation arrangement—the arbitrary readings so obtained being reduced to Centigrade temperatures afterwards, either by direct experimental comparison, or by calculation from the following formula,

$$t = N - \{(N - T)^2 - ke\}^{\frac{1}{2}},$$

where N is the neutral point of the two metals which form the joint (the rod itself would do for one), T is the temperature of the cool joint, which may be the same all the time, e the observed electromotive force on any arbitrary scale, and k a constant expressing the value of this scale determined once for all by an observation with a known value of t .

The only satisfactory method of heating is by the boiling of some liquid or the condensing of its vapour. Water vapour is scarcely hot enough, even under pressure; but oil might do. The heat should be conducted into the vacuum by a thicker

copper rod connected with the experimental rod, so that observations of temperature may be taken in the hotter as well as in the cooler portions of the rod.

Errata in No. 42 (March 1879).

Page 207, equation after eq. (6), for $\frac{2Pa^{v_0}}{(\log a)^2}$ read $\frac{1}{2}Pa^{v_0}(\log a)^2$.

— 209, eq. (8'), for 247 read 274.

Mr. Stocker, Physical Demonstrator in the Clarendon Laboratory, Oxford, has been good enough to point out to me, in addition to the above errata, that the value of $\log a$ is more nearly .0077 than .0076, as I have taken it, and that twice the reciprocal of this number, which in equation (7) is called 267, is more nearly equal to 260.

XLII. *The Theory of Binaural Audition. A Contribution to the Theory of Sound.* By ANTON STEINHAUSER.

[Concluded from p. 197.]

6. **T**HE results developed in the preceding theory, however, may be influenced by many circumstances—as, for example, by the conduction of sound through the earth or through the body, and particularly by the effect of reflexion. For if, in addition to the direct rays which produce the intensity i_1 in one ear and i_2 in the other, there reach the ear other indirect rays by reflexion from the ground, or walls &c., then the intensities with which the sound is perceived in the two ears become respectively $(i_1 + \rho_1)$ and $(i_2 + \rho_2)$, where the increments of intensity ρ_1 and ρ_2 may evidently be to one another in any ratio whatever, according to the existing circumstances. Then from equation (2) we get

$$\tan \alpha = \frac{(i_1 + \rho_1) - (i_2 + \rho_2)}{(i_1 + \rho_1) + (i_2 + \rho_2)} \tan \beta,$$

or

$$\tan \alpha = \frac{(i_1 - i_2) + (\rho_1 - \rho_2)}{(i_1 + i_2) + (\rho_1 + \rho_2)} \tan \beta. \quad . \quad . \quad (3)$$

Consequently the angle α is another one than that indicated either by the real direction of the source of sound, or by the projection of the direction of the sound-rays upon the plane of best hearing.

The angle at which we estimate according to the sensations the position of the source of sound (regarded no longer as necessarily in the plane of best hearing) approaches the more nearly to the angle at which it is actually situated in proportion as ρ_1 and ρ_2 are simultaneously diminished, and coincides with it when $\rho_1 = \rho_2 = 0$. This is almost attained in the case of weak sounds; for then the effects of reflexion, usually still weaker, are scarcely or not at all perceived.

It is possible, for example, to conduct researches upon pure direct hearing, uncomplicated by effects of reflexion, by means of a watch removed to as great a distance as is convenient and held by some person in any place he chooses, whilst the observer attempts with closed eyes to discover its direction by its ticking. Moreover the direct action of the sound will not be injuriously affected by effects due to reflexion, provided

$$(i_1 + \rho_1) : (i_2 + \rho_2) = i_1 : i_2;$$

for then

$$\frac{(i_1 + \rho_1) - (i_2 + \rho_2)}{(i_1 + \rho_1) + (i_2 + \rho_2)} = \frac{(i_1 - i_2) + (\rho_1 - \rho_2)}{(i_2 + i_1) + (\rho_1 + \rho_2)} = \frac{i_1 - i_2}{i_1 + i_2};$$

and therefore, if we substitute in equation (3) the value thus found for

$$\frac{(i_1 - i_2) + (\rho_1 - \rho_2)}{(i_1 + i_2) + (\rho_1 + \rho_2)},$$

we again obtain the equation

$$\tan \alpha = \frac{i_1 - i_2}{i_1 + i_2} \tan \beta,$$

in which α indicates the direction in which the source of sound is *actually* situated.

But from the proportion stated above we may deduce

$$i_1 i_2 + i_2 \rho_1 = i_1 i_2 + i_1 \rho_2,$$

or

$$i_2 \rho_1 = i_1 \rho_2;$$

therefore the proportion holds good,

$$\rho_1 : \rho_2 = i_1 : i_2.$$

Hence the power of estimating the direction is not affected by the effects of reflexion, provided the increments of intensity thereby occasioned in the two ears are respectively proportional to the intensities produced by the sound directly.

It will now be considered in the calculations which follow whether effects of reflexion produced by a single plane surface (say, for example, a vertical wall) can in certain cases fulfil the conditions mentioned above—that is to say, whether a position of the wall can be discovered in which the increments of intensity ρ_1 and ρ_2 arising from reflexion are respectively proportional to the intensities directly produced.

As in the previous figures, let AA' in figure 7 represent the line of sight, f_1 and f_2 the effective surfaces of the pinnæ, each including with it the angle β , α the angle included between the line of sight and the direction of the rays of sound S , which impinge upon both the reflecting wall W and the two

pinnae; lastly, let the angles which the line of sight makes with the wall and with the reflected rays be respectively ϕ and α_r .

We have, then, the following relations, amongst others, between these quantities:—

$$\alpha = \phi + \psi, \quad \psi = \alpha - \phi;$$

also

$$\alpha = \alpha_r + 2\psi = \alpha_r + 2\alpha - 2\phi,$$

and

$$\alpha_r = 2\phi - \alpha.$$

And, according to what has preceded, the ratio between the intensities produced by the direct action of the rays of sound when they make an angle α with the line of sight is

$$i_1 : i_2 = m : n = (\tan \alpha + \tan \beta) : (\tan \beta - \tan \alpha).$$

Hence, further, it follows that by analogy the ratio between the intensities produced indirectly by reflexion, when the reflected rays of sound make the angle α_r with the line of sight, must be

$$\rho_1 : \rho_2 = (\tan \alpha_r + \tan \beta) : (\tan \beta - \tan \alpha_r),$$

$$\rho_1 : \rho_2 = [\tan (2\phi - \alpha) - \tan \beta] : [\tan \beta - \tan (2\phi - \alpha)].$$

That these ratios shall be equal requires that $\rho_1 : \rho_2 = i_1 : i_2$, or that

$$\begin{aligned} [\tan (2\phi - \alpha) + \tan \beta] : [\tan \beta - \tan (2\phi - \alpha)] \\ = (\tan \alpha + \tan \beta) : (\tan \beta - \tan \alpha), \end{aligned}$$

or that

$$2 \tan \beta : 2 \tan (2\phi - \alpha) = 2 \tan \beta : 2 \tan \alpha;$$

from which it follows that

$$\tan (2\phi - \alpha) = \tan \alpha.$$

But since $\tan (2\phi - \alpha)$ may equal $\tan \alpha$ either when $2\phi - \alpha = \alpha$ or when $2\phi - \alpha = 180^\circ + \alpha$, we have two values for ϕ , namely $\phi = \alpha$ and $\phi = 90^\circ + \alpha$ respectively; but of these the first alone has any meaning in the case under consideration, since, as we see from simple inspection of figure 7, ϕ cannot have a value greater than α if a reflexion is to be possible.

Hence the estimate of direction will be unaffected by the reflexion at a single vertical wall only if that wall runs parallel to the direction of the rays of sound.

The direct rays reach both ears so long as $\alpha \leq \beta$. Similarly also the indirect rays reflected at the wall W will reach both ears so long as $\alpha_r \leq \beta$. For, since $\alpha = \alpha_r + 2\psi$, as may be seen from figure 7, it follows that $\alpha_r < \alpha$ always; and hence $\alpha_r \leq \beta$ as long as $\alpha \leq \beta$.

The rays reflected at the wall W will therefore reach both ears so long as this is the case with the direct rays.

We have then obtained the following measures for reckoning the rays of sound which reach the left and right pinnæ respectively :—

(1) for *direct* rays, the expressions

$$f \sin (\alpha + \beta) \text{ and } f \sin (\beta - \alpha);$$

(2) for *indirect* rays reflected at the wall W,

$$f \sin [2\phi - \alpha + \beta] \text{ and } f \sin [-2\phi + \alpha + \beta].$$

Consequently we have, as a measure for the total number of rays of sound which reach the left and right pinnæ respectively, the expressions

$$\{f \sin (\alpha + \beta) + f \sin [2\phi - \alpha + \beta]\}$$

and

$$\{f \sin (\beta - \alpha) + f \sin [-2\phi + \alpha + \beta]\},$$

provided the wall W is regarded as a completely reflecting surface.

Therefore the intensities i'_1 and i'_2 , with which the sound is perceived in the left and right ears respectively in virtue of the united direct and indirect actions, will be to one another in the ratio of the two expressions just determined, and give the following equations :—

$$\begin{aligned} \frac{i'_1}{i'_2} &= \frac{f \sin (\alpha + \beta) + f \sin [2\phi - \alpha + \beta]}{f \sin (\beta - \alpha) + f \sin [-2\phi + \alpha + \beta]} \\ &= \frac{\sin (\alpha + \beta) + \sin [2\phi + (\beta - \alpha)]}{\sin (\beta - \alpha) + \sin [(\alpha + \beta) - 2\phi]}, \end{aligned}$$

whence

$$\begin{aligned} \frac{i'_1 - i'_2}{i'_1 + i'_2} &= \frac{\sin (\alpha + \beta) - \sin (\beta - \alpha) + \sin [2\phi + (\beta - \alpha)] - \sin [(\alpha + \beta) - 2\phi]}{\sin (\alpha + \beta) + \sin (\beta - \alpha) + \sin [2\phi + (\beta - \alpha)] + \sin [(\alpha + \beta) - 2\phi]} \\ &= \frac{2 \cos \beta \sin \alpha + 2 \cos \beta \sin (2\phi - \alpha)}{2 \sin \beta \cos \alpha + 2 \sin \beta \cos (2\phi - \alpha)} \\ &= \frac{\cos \beta [\sin \alpha + \sin (2\phi - \alpha)]}{\sin \beta [\cos \alpha + \cos (2\phi - \alpha)]} \\ &= \cotan \beta \tan \phi \\ &= \frac{\tan \phi}{\tan \beta}. \end{aligned}$$

The direction in which, then, the source of sound is esti-

mated is defined by the equation

$$\tan \alpha' = \frac{i'_1 - i'_2}{i'_1 + i'_2} \tan \beta,$$

or, replacing the fraction by its value just found,

$$\tan \alpha' = \tan \phi.$$

And hence it must follow that $\alpha' = \phi$, since the second possible value of α' , namely $180^\circ + \phi$, is inapplicable to the problem, as is readily seen.

The effect which, in binaural hearing, a completely reflecting vertical wall exercises upon the perception of direction consists in this—that the observer of the source of sound seeks it always in the direction of the reflecting wall, independently of the direction in which it may be actually situated.

For example, let the source of sound be estimated to be situated in the line of sight, then must $\alpha' = 0$ and also $\phi = 0$. This shows that the source of sound will *always* be estimated and sought for in the line of sight if the completely reflecting wall runs parallel to this direction.

Again, let the source of sound be estimated to lie on the other side of the line of sight to that in which it is really situated, and in a direction making an angle with the line of sight equal to that which it really incloses; then obviously $\alpha' = -\alpha$ and also $\phi = -\alpha$.

Finally, let the wall W so reflect the sound that no illusion thereby affects the perception of the direction; then $\alpha' = \alpha$ and $\phi = \alpha$, which result coincides with that already found for this case.

Now imagine in figure 7 an additional second reflecting wall W' , vertically placed, and making with the line of sight the angle ϕ' , and let us investigate whether this cannot be so placed that the power of perceiving the direction will not be injuriously affected by a single reflexion of the sound at each wall. We should find, after a rather troublesome process of development, that the required condition will only be fulfilled if

$$\tan (\phi + \phi') = \tan 2\alpha,$$

which, in consequence of the existing circumstances, can again only be true if $\phi = \phi' = \alpha$. In that case both walls are parallel to the direction of the source of sound, and hardly any reflexion takes place at their surfaces.

Therefore the perception of the direction will be unaffected by two vertical reflecting walls only when these run parallel to the direction of the rays of sound.

The results developed in the preceding paragraph concern-

ing the effects of reflexion on direct binaural hearing may possibly not entirely coincide with the phenomena actually exhibited, since the very conditions assumed as fundamental in the argument are such as cannot in actual practice occur all at once. Thus the rays of sound which fall upon the surfaces of the two pinnæ and upon the reflecting wall were taken as parallel to one another. Results obtained upon this assumption are consequently only approximately correct when the source of sound is remote from the hearer, and when the surface (now not necessarily considered "large") of the wall which reflects the rays of sound to the ears lies very near to the hearer, since only under these circumstances can the rays which strike the pinnæ and also the surface of the wall be regarded as at all nearly parallel.

Lastly, the reflecting walls were assumed to be completely reflecting; that is to say, it was assumed that the rays falling collectively upon the surface of the wall were sent off in straight lines parallel to one another. Since this is in actual experiment untrue, for the rays of sound suffer a partial diffusion, as do the rays of light at an imperfectly polished surface, this circumstance implies a loss of the scattered rays, which may be greater or less according to the nature of the reflecting surface.

7. In concluding the subject of direct binaural hearing, we have only to add a few words on the perception of the change of place of the source of sound by the sensations of sound which are experienced when the head is not moved.

Let us imagine a source of sound which completes a motion within the region in which direct binaural hearing can occur, and let us inquire whether, and in what way, the nature of the motion can be learned from the sensation of sound. We shall, on consideration, easily find the answer to this question on referring to the theory developed in the preceding pages, as follows :—

The change of place of the source of sound has two consequences:—

(1) A change of the direction in which the rays of sound reach the ears, and therefore a change in the *ratio* between the intensities i_1 and i_2 with which the sound is perceived in the two ears.

(2) A change in the *total intensity* ($i_1 + i_2$) with which the sound is perceived.

The latter effect may result partly from a possible change of the distance of the source of sound from the observer, and partly from the change of the direction of the rays of sound.

Disregarding the latter circumstance—for, as will presently

be shown, the change in the total intensity ($i_1 + i_2$) caused by the change in direction of the rays of sound is extremely small—then, by what has preceded, we may conclude *from the new ratio* between the intensities i_1 and i_2 what the new direction is in which the source of sound is situated, or, in other words, what the motion of the source of sound *to right or left* may be; and we may also conclude by the change of the total intensity what the change of distance is, or, in other words, what the motion of the source of sound *backwards or forwards* may be. *It follows that every change of place of a source of sound situated within the region of direct binaural hearing may be known as to its nature by the hearing of the two ears.*

For, to recapitulate, the change of place of the source of sound is known in respect of right and left from the new ratio $i_2 : i_1$, and in respect of forward or backward by the new sum $i_1 + i_2$.

From figure 3 we see that the total number of rays of sound reaching both ears may be measured by the length of the straight line de . Now this is *longest* when the direction of the rays of sound coincides with the line of sight, indicated in figure 8 as S_1 ; but it is *shortest* when the rays of sound take the direction of the line S_2 (fig. 8). Hence it follows that the ratio between the greatest and least of the possible values of the total intensity, namely $J_g : J_l$, must be equal to the ratio between the straight lines de and gk . But since $de = bc$ and $gk = bv$, we have the proportion

$$J_g : J_l = bc . bv,$$

or, lastly, since $bv = bc . \cos \beta$,

$$J_g : J_l = 1 : \cos \beta.$$

If we take for β its greatest possible value, namely about 30° , in order that the difference between the two members of the ratio may become a maximum, it follows that

$$J_g : J_l = 1 : 0.866,$$

whence we infer that *the change in the total intensity with which a sound is perceived occasioned by a change of the position of the source of sound* (supposed always to be situated within the region of direct binaural hearing) *is very small.*

2. The Theory of Indirect Binaural Audition.

8. If a source of sound is situated in the region of indirect binaural hearing, *no* ray of sound can reach the surface of either of the pinnae directly; and the sound produced by the sonorous body can evoke a sensation *only as the result of*

reflexion, provided we neglect the possible conduction of sound through solid bodies.

Whether that operation puts us in a position to form a judgment concerning the direction in which the source of sound is situated, will be seen from what follows.

Let AA' in figure 9 be the line of sight, f_1 and f_2 the effective surfaces of the pinnæ for rays of sound turned by reflexion at the surface W from the direction S into the direction s , α the angle which the rays of sound make with the line of sight *before* reflexion, and α_r the angle they make after reflexion. Then it is readily seen that, since neither of the surfaces of the pinnæ can be directly reached by the rays S , the hearer will receive an impression identical with that which he would receive if the source of sound were situated in front to the left in the direction of the reflected rays s . Now it follows from figure 9 that

$$\phi = \alpha_r + \gamma \quad \text{and} \quad \alpha = \phi + \gamma,$$

and that

$$\alpha_r = \phi - \gamma, \quad \text{and} \quad \gamma = \alpha - \phi.$$

Substituting for γ its value in the equation $\alpha_r = \phi - \gamma$, we have

$$\alpha_r = \phi - \alpha + \phi,$$

or

$$\alpha_r = 2\phi - \alpha.$$

That which is heard, therefore, *indirectly* in the direction α makes the same impression as that heard *directly* in the direction α_r ; in which case α_r , whose value is dependent on ϕ , may *without any change of the direction of the rays of sound* S , assume an indefinite number of different values, since the position of the reflecting surface may as well be any other than it is, or there may be many reflecting surfaces.

And since, for all remaining directions of rays of sound, the effect upon the ear may be radically different according to the nature of the surrounding reflecting surfaces, no conclusion can be drawn from this effect as to the direction in which the source of sound is situated: indeed it raises very illusory suggestions; for we suppose the source of sound to be in a particular direction in front in the region of direct binaural hearing, whereas it is situated behind in the region of indirect hearing.

These illusions of the source of sound being apparently situated behind instead of before, do not appear always, nor even very frequently, and may seem quite different in different cases, as is proved both by the sensations which at almost every moment impress themselves upon the attentive observer and

by experiments which may be very readily made with respect to this point.

The cause of this, however, lies in the particular circumstances which unconsciously influence the estimate of the direction. The most important of these circumstances are :—

(1) *Trains of thought* suggesting a certain direction as being that in which a source of sound is constantly situated.

To give one single example :—Suppose while walking along a road a person hears himself called by name, he will most naturally conjecture that the person calling is behind him ; and conjectures thus, not only for the reason that he does not see the speaker in front of him, but also specially because the very purpose of so calling is usually to bring to a standstill the person called, so that he may be more easily overtaken.

(2) *A certain indistinctness* which generally characterizes the indirect perceptions of sound. This arises from the circumstance that the indirect sensations of sound are nothing less than a whole series of echoes following one another, and more or less running into one another, producing a lengthening-out of every single element of sound, so that the last echoes of one element of a sound may coincide with the first echoes of the element immediately following it.

Thus, for example, in hearing sounds which come from behind upon an open plain, as in figure 10, the sound of the voice of the speaker S (the arrows indicate the line of sight) can only reach the hearer H by reflexion at the surface of the earth, which is never perfectly level. Hence a sound of infinitely short duration proceeding from S will be drawn out to a considerable length, since the rays of sound reflected from the more distant points of the earth's surface do not arrive simultaneously but reach the hearer in rapid succession.

Now it may happen that either, as is the case when the earth has an equably rough surface, the later echoes are weaker, or, as when there are reflecting surfaces, such as hills or houses present, the later echoes are stronger than those which precede them.

The total impression of an indirectly perceived momentary sound, such as a crack, will in the former case be a dying out or ceasing to sound, in the latter case an irregular decreasing and swelling again with a rolling effect.

This last phenomenon is perceived when thunder is heard, in which all the sudden claps occurring during the progress of a prolonged peal imply each a reflecting surface such as a mountain, causing great volumes of sound simultaneously reflected to arrive at certain irregular intervals. Similar phenomena occur also after the firing of a cannon in a mountainous neighbourhood.

From what has been said, it might readily be imagined that a direct perception of sound can be discriminated from an indirect one more readily in the cases where the sound produced by the sonorous body is a familiar one.

Thus, for example, by the action of an echo, the familiar voice of a friend, or the familiar roll of a waggon, with the regular tramp of its horses, will suffer a certain indefinable change scarcely consciously perceived, from which we shall conclude, either from experience or conjecture, that the source of sound is situated in the region of indirect binaural hearing, and therefore behind us.

This discrimination becomes far more difficult or even impossible if the source of sound and its varying tones be unfamiliar; for if the sound be of a uniform character, such as the tone of an organ-pipe, the buzzing of an insect, the various elements of sound which by their united combination would produce echoes occur at different times, and hence can exert no such destructive effect on the character of the sound. But discrimination is most difficult of all when the sound is of an undecided or irregular nature, in which the separate elementary sounds burst forth one after the other, as, for example, in the sound of cattle-bells.

As there are no other circumstances of any importance besides those which have been adduced that aid in the perception of the direction in which a source of sound is situated, and as these moreover afford no exact estimate of the direction, we conclude that *in the most favourable cases we are only in the position to decide that the source of sound is situated in the region of indirect binaural hearing.*

If we would determine with any degree of exactness the direction in which the source of sound is situated in such a case, we must, as figure 11 shows, place before the ears f_1 and f_2 , and parallel to them the planes h_1 and h_2 ; for these planes would reflect the rays arriving in the direction S so as to produce an impression which would coincide with that which would be produced by a source of sound situated in the reversed direction, S.

9. As already previously remarked, *direct* sensations of sound almost never occur without some admixture of indirect sensations.

It might therefore be contended that what has been said about the indirect perception of sounds might also be equally applied to their direct perception, and that consequently the alleged distinction between the two series of perceptions was no distinction at all. This objection may be met as follows:—

Suppose two sounds produced simultaneously at two diffe-

rent points, but such that their respective "timbres" or characters are somewhat different. We find by experience that the *single* ear does *not* sum up the impressions received from the two sources, but that the conceptions evoked by the impressions received in the ears from the one source of sound are *separate* from those evoked by the corresponding impressions received from the other source of sound, so that the presence of two separate sources of sound is known right well.

This severing of the separate perceptions of sound becomes, however, more difficult the more nearly the sources of sound resemble one another.

But that the ear is accustomed to such an analysis of the sensations, and that its analyzing-power may be whetted by practice, is proved by the power acquired by the conductor of an orchestra, who simultaneously follows the sounds of every separate instrument. Again, imagine, as in figure 12, that there is but one source of sound S, but that, in consequence of reflexion at the wall W, indirect rays of sound also reach the ears. Then the united effect of these direct and indirect rays is obviously equivalent to that which would be produced by two sources of sound, one situated at S, the other at S_1 .

But the ear, by greater attentiveness and practice, resolves these two sounds, which apparently proceed from two separate sources, and which reach it in quick succession, and in resolving them allows itself to be led to form a perception of the direction by the direct effect alone, passing the indirect effect by unnoticed.

If, on the contrary, this analysis is *not* achieved, by reason of want of attention or of practice, or in consequence of defective knowledge as to the finer characteristics of the direct sound (which an echo never completely repeats, and in consequence of which singular changes in the tone of the echo may be noticed), then acoustic illusions may occur, several examples of which have been already mentioned in the preceding paragraphs.

The ear, however, is no mere physical apparatus, but a sensitive organ of mind capable of being trained.

3. Theory of Mixed Binaural Audition

10. If a source of sound is situated in the region of mixed binaural hearing, then, as is known, the direct rays of sound can reach only *one* of the two pinnæ, while *both* may be reached by the indirect rays.

Accordingly let i_1 be the intensity with which the direct rays of sound affect (say) the left ear, and ρ_1 the increment of that intensity due to the effect of reflexion. Let ρ_2 be the intensity of the sensation in the right ear, and due to the re-

flexion *alone*. Then following the assumptions previously made, on summing up the indirect and direct effects, we obtain from equation (3),

$$\tan \alpha = \frac{i_1 + \rho_1 - \rho_2}{i_1 + \rho_1 + \rho_2} \tan \beta ;$$

and hence by calculation we can find the angle within the region of direct binaural hearing in which the source of sound is *erroneously* imagined to lie.

The angle α becomes equal to the angle β when ρ_1 and ρ_2 become simultaneously zero, or indeed when $\rho_2 = 0$, which may be stated as follows :—*If a source of sound be situated in the region of mixed binaural hearing, we seek it always in front of us, and in the direction of a line drawn through the surface of the pinna on which the direct rays do not fall, provided either that the indirect rays are of null intensity, or that they reach the other pinna only.*

Again, if the indirect effects were known as such, and were separated from the direct effects, as occurs already in the theory of indirect binaural audition, a means of making an exact estimate of the direction of a source of sound would still be wanting ; but still it is possible by experience to perceive that the source of sound is situated in one of the two regions of mixed hearing, from the circumstance that only one ear is reached by direct rays, and the other is reached by none but indirect rays.

It is situated, therefore, in the region of mixed hearing, respectively on the left and right sides, when *direct* rays enter only the left ear or only the right ear, and when none but *indirect* rays enter the left ear or the right ear respectively.

Moreover, in the case of mixed binaural hearing, it is possible to determine *the region* only, but not the direction, in which the source of sound is situated.

CONCLUDING REMARKS.

11. In order to facilitate a survey of the more essential results of the foregoing theory of binaural audition, they are recapitulated and briefly set forth in the following paragraphs.

The whole 360° of the region of binaural hearing divides itself into three portions :—*in front*, the region of direct hearing ; *at the two sides*, the regions of mixed hearing ; and *at the back*, the region of indirect hearing.

Under favourable circumstances, and without moving the head, we are, in consequence of our *binaural hearing*, able to decide from the sensations of sound in which region of hearing the source of sound is situated.

And exact knowledge of the direction in which the source of sound is situated, and an estimate of any motion executed

by it, are only possible so long as the source of sound remains in the region of *direct* binaural hearing.

If, on the other hand, the source of sound is situated either in the region of indirect or in that of mixed binaural hearing, we may arrive at an exact knowledge of the direction in which it is situated by turning the head while we seek to bring the source of sound:—

(1) Into the region of direct hearing, and preferably into the direction of best binaural hearing.

(2) Onto the boundary of two neighbouring regions of hearing. Since the direction of this boundary is known to each individual, the direction of the source of sound when brought into coincidence with this direction is also known. This auxiliary means offers in many cases the advantage that it only requires a slight movement of the head. If, for example, the source of sound be in the region of indirect hearing, a very slight movement of the head suffices to bring it onto the boundary between the regions of indirect and of mixed hearing.

(3) Into the known direction of best monaural hearing. This happens when in doubtful cases we wish to decide whether the source of sound is situated in front or behind. For if we turn the head about a vertical axis, according as the source of sound is situated before or behind, the intensity of the sound will increase in the ear that is turned forward or backward respectively, since by the movement of the head the source of sound is brought more nearly or even quite into the direction of best hearing for one of the two ears.

As may also be deduced from the preceding theory, the angle β which the surfaces of the pinnae make with the line of sight, may, in certain circumstances, exercise no inconsiderable influence upon binaural hearing. Since this angle is not always alike in different individuals, but may differ within certain tolerably wide limits, the consequences which the largeness or smallness of that angle may involve, are here summarily enumerated.

For, in accordance with the preceding theory, the larger the angle β is,

(1) the wider will be the region of direct binaural hearing,
 (2) the smaller will be the region of mixed binaural hearing,
 (3) the better and more distinct will hearing be in the line of sight,

(4) the larger will be the region of indirect binaural hearing,
 (5) the less certain will be the estimation of direction in the region of direct binaural hearing, and, lastly,

(6) the greater will be the possible difference in the total

intensity with which a sound is perceived for the various positions of the head.

The circumstances adduced in the first three points may be regarded as the *advantages*, those in the last three as the *disadvantages* entailed by relatively increasing the value of the angle β .

12. In conclusion let us finally draw attention to the fact that estimates of direction may be made in many cases in which, according to the preceding theory, the faculty of so doing is strictly wanting to the organs of hearing. Without entering further upon the circumstances involved by this apparent difficulty, be it simply remarked that often the most inconsiderable and apparently unimportant things are accustomed to suggest a particular position as that of the spot in which the source of sound is situated, so that then this power of finding the direction is falsely ascribed to the ear.

Just in this manner, as estimations of depth or distance can theoretically be made by *one* eye in a very defective way only, and yet are again and again easily made owing to manifold circumstances which stand in no direct connexion whatever with the faculty of adjustment of the eye, so also estimations of direction of sounds are made by the aid of other circumstances without recourse to the particular faculties of the ear.

Moreover, the more excitable the imagination of man is, the more easily will he see and hear things at a place where really there is nothing to see and nothing to hear.

XLIII. *On a new Absolute Galvanometer.* By N. D. C. HODGES, Assistant in the Physical Laboratory of Harvard College, Cambridge, Mass.*

IN the ordinary form of galvanometer the current is measured by the ratio of the force it exerts on the needle to the directive force of the earth, the ratio being determined by a measurement of the angle of deflection.

The moment of the force with which a unit current acts on the needle may be expressed in a series of the form

$$G_1 g_1 \sin \theta + G_2 g_2 \sin \theta Q'_2(\theta) + \&c. \text{ (Maxwell, § 109).}$$

G_1 , G_2 are constants depending on the dimensions of the coil, and g_1 , g_2 on those of the suspended apparatus, coil or magnet, as the case may be. $Q'_2(\theta)$, $Q'_3(\theta)$ are quantities which may vary with the deflections.

Only in case all the terms after the first may be neglected

* Communicated by the Author.

are the values of the current proportional to the tangent of the deflection. With a single coil this is not the case. By increasing the number of coils and suitably placing them, the magnetic field may be rendered more uniform.

In reading the deflection, either a divided circle or a telescope and scale are used. With the divided circle the deflection may be as great as $45'$, but not more, or else the instrument would not be sensitive to changes in the current. The use of telescope and scale necessitates much smaller deflections. To regulate the strength of the current, shunts of small resistance often have to be used, and render the proportion of the current through the instrument doubtful.

If, instead of placing the plane of the coils parallel with the magnetic meridian,³ they are placed perpendicular to it, the sum of the force of the current and of the directive force of the earth would influence the magnet.

The formula

$$\frac{t^2}{\pi^2} = \frac{K}{MT(1+\theta)} \cdot \cdot \cdot \cdot \cdot \quad (1)$$

expresses the relation between the time of oscillation of a magnet, its magnetic moment M , its moment of inertia K , and the horizontal component of the earth's magnetic force T .

If the current is passing, the following formula will hold:

$$\frac{t_1^2}{\pi^2} = \frac{K}{M(T+F)(1+\theta)}, \cdot \cdot \cdot \cdot \cdot \quad (2)$$

F being the force due to the current. From (1) and (2),

$$\frac{t^2 - t_1^2}{t_1^2} = \frac{F}{T} \cdot \cdot \cdot \cdot \cdot \quad (3)$$

The moment of the force F on the magnet is

$$C(G_1 g_1 \sin \theta + G_2 g_2 \sin \theta Q'_2(\theta) + \&c.) :$$

θ is the angle between the axis of the coil and of the magnet ; and C is the strength of current. From this

$$C(G_1 g_1 + G_2 g_2 Q'_2(\theta) + \&c.) = F.$$

For the small angles through which the magnet need vibrate, the second factor of the first term may be considered constant, and equals the constant of the instrument used as a tangent-galvanometer when the deflection is supposed equal to 90° . Let

$$G_1 g_1 + G_2 g_2 Q'_2(0^\circ) + \&c. = K_{90^\circ},$$

and

$$g_1 + G_2 G_1 g_2 Q'_2(\theta) + \&c. = K_\theta,$$

where

$$\phi = 90^\circ - \theta.$$

Then

$$F = CK_{90^\circ}.$$

Substituting this value of F in (3), we get the expression for C ,

$$C = \frac{t^2 - t_1^2}{t_1^2} \frac{T}{K_{90^\circ}} \dots \dots \dots (4)$$

To find the value of K_{90° , any of the ordinary experimental methods may be used; or, if the constant for any value of ϕ is known, when the same coil and magnet are used as a tangent-galvanometer, it might be obtained as follows:—Having a constant current, get its value in terms of K_ϕ and the tangent of the deflection, and then in terms of the quantities in formula (4).

$$C = \frac{T}{K_\phi} \tan \phi;$$

$$C = \frac{t^2 - t_1^2}{t_1^2} \frac{T}{K_{90^\circ}};$$

$$\therefore K_{90^\circ} = \frac{t^2 - t_1^2}{t_1^2} \frac{K_\phi}{\tan \phi},$$

.....

It is evident that the relative values of K_ϕ for the different values of the deflection of a tangent-galvanometer may be found by a repeated application of this process by the use of currents of different strengths.

$$K_{90''} = \frac{t^2 - t_1^2}{t_1^2} \frac{K_\phi}{\tan \phi};$$

$$K_{90^\circ} = \frac{t^2 - t_2^2}{t_2^2} \frac{K_{\phi_1}}{\tan \phi_1};$$

$$\therefore \frac{K_\phi}{K_{\phi_1}} = \frac{\tan \phi}{\tan \phi_1} \frac{t^2 - t_1^2}{t_1^2} \cdot \frac{t_2^2}{t^2 - t_2^2},$$

.....

Physical Laboratory, Harvard College,
Cambridge, U. S. A., March 1, 1879.

XLIV. *A new Determination of the Ratio of the Electromagnetic to the Electrostatic Unit of Electric Quantity.* By W. E. AYRTON and JOHN PERRY *.

[Plate XI.]

Introduction.

THE fact that metals had a different power of conducting electricity was discovered by Sir Humphry Davy in 1821 †, although the idea of resistance as a property of a conductor was not introduced until the publication of Ohm's law in 1827, in which a resistance was first regarded as a magnitude. Now a magnitude necessarily implies a unit of measurement; but the earlier writers merely contented themselves with reducing by calculation the resistance of all parts of a heterogeneous circuit into a given length of some given part of that circuit; so that they generally spoke of the resistance "as the reduced length of the conductor."

The next step was naturally to refer these "reduced lengths" to the length of some standard wire which might perhaps not be employed in the circuits under test, and to consider the resistance of unit length of this standard wire as the unit resistance. Consequently we find the unit which was employed by Lenz, in 1838, to be defined as that of 1 foot of No. 11 copper wire, and the unit of Wheatstone, in 1840, as that of 1 foot of copper wire weighing 100 grains. Until the year 1850 measurements of resistance were confined, with few exceptions, to the laboratory; but about that time underground wires, followed shortly after by submarine cables, began to be employed; and when on these new lines it was no longer possible to determine the position of a fault by inspection, an intimate knowledge of the laws of electricity, combined with an accurate standard of resistance, became of great practical importance to the telegraph-engineer. The unit of length in the laboratory, "the foot," was replaced in construction by "the mile;" thus the unit of resistance in England became that of a mile of no. 16 copper wire, and in France that of a kilometre of iron wire 4 millimetres in diameter. Several other units were from time to time proposed, of which two, that of Weber and that of Thomson, differed altogether from the others in their fundamental conception. Excluding the two last, all the units of resistance proposed were based on the obstruction offered to an electric current by a given length of a given material,

* Communicated by the Authors, having been read before the Society of Telegraph Engineers, February 26.

† "Report to the Royal Society on the new Unit of Electrical Resistance, &c.," by Prof. F. Jenkin.

of a given section, at a given temperature ; but as soon as it had been ascertained that a comparatively slight trace of certain impurities introduced into a conductor seriously affected its specific resistance, it became clear that no one of the previously proposed standards was sufficiently definite. Consequently Jacobi, in 1848, felt it necessary to send to Poggendorff and others a certain copper wire, since well known as "Jacobi's standard," in order that electric copies of it might be taken, to avoid the growing inconvenience of the multiplicity of standards.

But measurements of resistance can be conceived and carried out entirely without reference to the special qualities of any material whatever ; and in 1849 Kirchhoff had effected a measurement of this nature : it was not, however, until 1851 that Weber proposed a distinct system of measurement based on the fundamental units of length, mass, and time, and such that electrical resistance according to it would be expressed by an absolute velocity. Previous to this, Gauss, desiring to obtain precise measurements of terrestrial magnetism at different parts of the earth's surface, found it necessary at the outset to decide on a unit of force which was not, like the weight of a pound, affected by the position of the place in which the experiment was made. He therefore devised what has since become well known as Gauss's "absolute" or relative unit of force, based on the fundamental units of length, mass, and time. In accordance with this nomenclature of Gauss, Weber called his method of electrical measurement the "absolute electro-magnetic" system. As soon as the proposed system of Weber appeared, Thomson accepted and extended it, showing that the unit of absolute work, the connecting link between all physical forces, formed part of the same system ; consequently the units of resistance of Weber and Thomson were not based on the physical properties of any special substance, but merely on the fundamental units of length, mass, and time.

Mention must not be omitted of the mercury unit of Siemens, since, although not an absolute one, the coils and apparatus constructed by Dr. Siemens were made with such care that his system has materially helped in obtaining the present accuracy of the standards issued by the Committee of the British Association.

In addition to the vagueness introduced by selecting the resistance of a special rod of some material as our standard, there would be the consequent necessary introduction of various numerical coefficients into the equations connecting current, resistance, electromotive force, work, &c. It was

therefore though desirable by the Committee of the British Association, when appointed in 1861 to consider the question of the selection of electrical units, that they should decide on some system not only independent of any particular material, but also of such a nature that every simple equation connecting the absolute measurements of force, work, electric and magnetic quantity, current, resistance, and electromotive force should be independent of numerical coefficients.

But there are, as is well known, six fundamental equations connecting these six quantities—

$$1. \quad f = \frac{q_1 q_2}{d^2},$$

$$2. \quad f = \frac{m_1 m_2}{d^2},$$

$$3. \quad q = ct,$$

$$4. \quad f = \frac{clm}{d^2},$$

$$5. \quad w = c^2 rt,$$

$$6. \quad c = \frac{e}{r},$$

one equation in fact more than is necessary to produce a single system of units. The consequence is that one or other of the two similar equations, Nos. 1 and 2, connecting electric or magnetic quantity with force must be rejected. If we reject No. 2, then 1, 3, 4, 5, 6 determine the electrostatic units of quantity, current, magnetic pole, resistance, and electromotive force; and by rejecting No. 1, then 2, 4, 3, 5, 6 determine the electromagnetic units of magnetic pole, current, quantity, resistance, and electromotive force. The names "electrostatic" and "electromagnetic" refer of course only to the fundamental conception of the two systems, and do not in any way imply that in these systems the electricity must be in rest or motion: thus, for example, we may have an electrostatic unit of current.

Nature of "v," and the importance of Measuring its Value.

The object of the investigation described in this paper is to determine the value of "v," the ratio of the electromagnetic to the electrostatic unit of quantity; but it may be asked, since measurements in electromagnetic units are alone employed by telegraph-engineers, what interest has such an investigation or its results to them? The answer is, We cannot, from the nature of an electromotive force, have a standard cell of the

same constancy as a resistance-coil*; therefore, if any one desires to measure with great accuracy the electromotive force of his battery, he is not able to do this by a simple comparison with a standard cell, but he must determine it absolutely himself. Now the simplest way to do this, is to measure its *electrostatic* value with an absolute electrometer, and to convert the result into electromagnetic measure or into volts, by using the proper multiplier, which necessarily depends on the ratio v .

There are also other practical uses that may be made of this constant; but the main interest attached to the exact determination of its value consists in its constituting a test of the accuracy of the theory that the same medium which transmits the vibrations that constitute light transmits those also which produce electro-magnetic induction.

For the two units of electric quantity are of a totally different nature from one another. If, for example, you take a yard and a foot, two units of length, and divide the one by the other, you get the simple number three, or, if you take two units of weight, a ton and a pound, you get a simple number 2240; but if you divide the electromagnetic by the electrostatic unit of quantity, it is more like dividing a solid by an area: in that case you do not get for your quotient a number, or a solid, or an area, but a length. So the ratio of the electric units is not a number but a velocity †, and an absolute velocity in nature independent of the units of space and time; and Prof. Clerk Maxwell has proved that this velocity must be that of the propagation of electromagnetic disturbances in a non-conducting medium, or, assuming that light is an electromagnetic disturbance, must be equal to the velocity of light ‡.

1. Previous Measurements of " v ."

The first estimate of the relation between a quantity of electricity measured statically and the quantity transferred by

* The electromotive force of Mr. Latimer Clark's mercurous sulphate cell is undoubtedly very constant, but is necessarily altered by the shaking in travelling, by the presence, or absence, of free mercury in the paste, by the mode in which the mercurous sulphate paste is prepared, &c. In fact, as we have pointed out (Proc. Roy. Soc. No. 186, 1878), measurements of electromotive force are far more delicate than any chemical tests.

† The dimensions of a quantity of electricity measured electrostatically are $\left[\frac{L^{\frac{3}{2}} M^{\frac{1}{2}}}{T}\right]$, measured electro-magnetically $[L^{\frac{1}{2}} M^{\frac{1}{2}}]$; therefore the dimensions of the ratio is $\left[\frac{L}{T}\right]$ a velocity. (Jenkin, 'Electricity and Magnetism,' p. 164.)

‡ 'Electricity and Magnetism,' chapter xx.

a current in a given time was made by Faraday* ; but being measured in arbitrary units, as the absolute system was not then developed, Faraday's comparison gave no indication of the value of v .

There are several ways of measuring this value. The first numerical determination was made, in 1856, by Messrs. Weber and Kohlrausch† ; and their method was founded on the measurement of the same quantity of electricity, first in electrostatic and then in electromagnetic units. The result they obtained was 310·7 million metres per second.

But as the quantity they measured electrostatically was practically the amount discharged in a finite time by a Leyden jar previously electrified to a fixed difference of potentials, and the amount measured electromagnetically was the instantaneous discharge of the same Leyden jar electrified to the same difference of potentials, it is probable that the result of the first of these measurements and consequent value of v obtained was, on account of the electric absorption of the glass, rather too large.

The next determination of the value of v was made by Sir W. Thomson in 1868‡, who measured the same electromotive force electrostatically with his absolute electrometer, and electromagnetically by determining with an electro-dynamometer the electromagnetic value of the current sent by this electromotive force through a known resistance. The mean of eleven sets of experiments, from which the highest value obtained was 292 and the lowest 275, gave a result of 282·5 million metres per second.

In the preceding method two forces had to be separately measured, one by means of an electrometer, and the other with an electro-dynamometer ; but Prof. Clerk Maxwell, about the same time, carried out a method by means of which these two forces were made to balance one another, so that the ratio of the electrostatic and electromagnetic measures of the same electromotive force was obtained without previously ascertaining the value of each. The highest of the twelve most accurate results was 294, and the lowest 284, and the mean for v 288 million metres per second. In both these methods it was necessary to know the absolute resistance of a certain coil employed.

It will be observed that even the highest values obtained by either Sir Wm. Thomson or Prof. C. Maxwell were lower

* Experimental Researches, Series iii. § 362.

† C. Maxwell, 'Electricity and Magnetism,' ch. xix. p. 370.

‡ Sixth Report of the Committee of the British Association on Electrical Standards, 1869.

than the velocity of light, which is about 300 million metres per second, and their mean value far lower.

In 1872 a redetermination of the value of v was made by Mr. Dugald McKichan, in Sir Wm. Thomson's laboratory*, using the same method that Sir William had previously employed; but as certain improvements had since 1867 been introduced into the absolute electrometer, the results now obtained were more accurate. The mean value on this occasion was 293 million metres per second, still, however, being much lower than the velocity of light. It is important, however, to notice that some single values obtained were as high as 300 million metres, although these are again balanced by others as low as 290 million metres per second.

2. Method employed in this Investigation.

Now the velocities ascertained for light are:—

M. Fizeau	314	} million metres per second.
Aberration &c. and sun's parallax	308	
M. Foucault	298	
M. Cornu	300	

When, therefore, Professor Perry and myself, in 1877, took up the question experimentally, it could not be said that the ascertained value of v was equal to the velocity of light, although Professor Clerk Maxwell's electromagnetic theory required the identity for its corroboration.

In fact the best experiments seemed to show that v was, for some reason, less than the accepted velocity of light. The question therefore arose, was Professor Clerk Maxwell's theory incomplete, or was it that the accepted velocity of light was too high, or was it that the methods previously employed for the determination of v might be improved on, and a more correct value obtained? This leads to the consideration of what other methods than those employed by MM. Weber and Kohlrausch, Sir Wm. Thomson, and Professor Clerk Maxwell were available. Now it was possible to determine its value by an accurate comparison of the electrostatic capacity of a condenser with the electromagnetic capacity of self-induction of a coil†; but it seemed to us very doubtful, from the nature of the experiments, whether this method would give results more accurate than those previously obtained. And the same remark applied even with greater weight to the measure of a resistance electrostatically and electromagnetically, since the same difficulty would here have been met with that is encountered when it is desired to measure the insulation of a cable very

* Philosophical Transactions of the Royal Society, 1873, p. 409.

† Clerk Maxwell's 'Electricity and Magnetism,' p. 379.

(fig. 2) the same in plan. The plate is strengthened by stout brass ribs D F, E G (fig. 2), and A B, B C (fig. 1). L, L, L are three chemically cleaned and paraffined ebonite levelling-screws, the ends of which are thinned to a blunt point so as to allow extremely little surface leakage, and by means of which the plate A B C can be adjusted parallel to the lower plate, H J K (fig. 1), which is shown in plan (fig. 3) as L M N P. This plate is also strengthened by stout brass ribs underneath, L N, M P (fig. 3), and H J, J K (fig. 2). This lower plate, by means of hole-slot and plane, rests on three chemically cleaned and paraffined ebonite levelling-screws, *l, l*, by means of which its *upper* surface is made to exactly coincide with the top of the guard-ring Q R, S T (fig. 1), and U V W X (fig. 3). This guard-ring is rigidly soldered to the upper edges of the brass box *b b, b b* (figs. 1 and 3), three projections on the sides of which support, with hole-slot and plane, the levelling-screws L, L, L. Into the bottom of this box screw the ebonite levelling-screws *l, l*. Small vessels containing calcium chloride (not shown in the figure) are placed inside the brass box, *b b, b b*, to keep the ebonite in the neighbourhood of the ebonite levelling-screws *l, l* quite dry, in order to avoid the possibility of surface-leakage. In the earlier experiments the space between the edges H K of the lower plate and of the guard-ring R S (fig. 1) was very small; but afterwards, to avoid the possibility of leakage across by sparking or otherwise, this was increased to 2·5 millimetres, and the area of the lower plate thus reduced to 1323·14 square centimetres. The errors arising from the surfaces of the condenser-plates not being true planes were practically eliminated by capacity-experiments being made with successive adjustments of the condenser-plates, a different set of points in the upper plate being each time brought to the fixed distance from the lower one.

4. The Galvanometer.

The galvanometer employed was one constructed some time back by Messrs. Elliott from our own design. It had a resistance of 19,970 ohms at 21°·9 C. In ordinary use, when fitted with an astatic combination, four magnets being used top and bottom, one Daniell's cell would give, through a resistance of 600 megohms, a deflection of 130 scale-divisions on a scale about $1\frac{1}{2}$ metre apart. But this arrangement, with its aluminium vane, had far too much damping for being used ballistically. We therefore commenced by removing the vane and weighting the lower set of needles with pieces of brass so as to give it a barrel shape; but if the brass was light we found there was too much damping, and if heavy too little sensibility:

consequently all the numbers obtained for v prior to June 18, 1878, we rejected. We now built up two little magnetic balls, each consisting of twenty little magnets, previously magnetized to saturation and slightly separated from one another with pieces of zinc: in each ball all the magnets pointed one way; and the two balls were used to form an astatic needle. As it would have been difficult to make the entire sphere all of magnets, we finished it off with segments cut from a wooden sphere. Now these magnetic spheres gave us an astatic arrangement of considerable sensibility and without very much damping, the decrement, or the ratio of one swing of the galvanometer-needle to the next, being 1.274. About June 18 we made experiments using this astatic combination; but fearing that even this decrement was too far from unity, we took the needle down in the interval between the 18th and 23rd, and replaced the segments of the wooden sphere by segments of a small leaden hemispherical *shell*, thus getting a considerable moment of inertia without much extra weight on the fibre. The decrement was now found to be diminished to 1.1695; and with a periodic time for the swing of the needle equal to 39.5 seconds very consistent results were obtained.

It might at first appear that the amount of damping action was not very important provided it was known, seeing that Professor Clerk Maxwell gives, on p. 348 of his 'Electricity and Magnetism,' the complete formula for determining the capacity of a condenser by the swing of a galvanometer-needle with any amount of damping. In reality, however, this formula is developed on the assumption that the resistance of the air is for slow velocities directly proportional to the velocity; but since we know for large velocities it is proportional to the square, or higher powers, and since the law is not, of course, discontinuous, the resistance even for low velocities cannot be accurately proportional to the velocity; hence the only way to get perfectly correct results is to diminish the retardation arising from the air or other causes to nearly *nil*.

5. *Method of Experimenting.*

A current from 382 perfectly new porous pot Daniell's cells in series was passed constantly through a resistance A B (fig. 4): the difference of potentials at two points, A, C, was then employed to send a current through the shunted galvanometer and through a known resistance R; and the deflection obtained was, say, d_1 . Without in any way altering the adjustment of the galvanometer, the connexions were then arranged as in fig. 5. By means of the key K, the upper plate, U, of the condenser could be connected either with one pole of the battery

or with the other. The fork F turning on the pivot P consisted of two arms perfectly insulated from one another; the one f_1 connected with the point A of the resistance-coils, the other arm f_2 , which consisted of a piece of Atlantic-cable core with pointed paraffined ends (to prevent any surface leakage) was connected with one terminal of the galvanometer. The stiff wire w rigidly attached to the lower plate L of the condenser and passing through a hole in the bottom of the brass box $b b b b$ without touching it, could therefore, by turning the fork F, either (1) be connected with the pole A of the battery, or (2) left insulated, or (3) discharged through the galvanometer. Both surfaces of contact were platinized. (The same set of connexions might have been arranged with an ordinary "charge and discharge key," but with not such perfect absence of leakage; for the lever of such a key which is supported on ebonite pillars, and along which some surface leakage must have taken place, would have had to be connected with the wire w .) The box $b b b b$ and the other pole of the galvanometer were permanently connected with A, which was joined to earth.

A complete experiment was as follows:—

1. f_1 pressed against w and K pressed down, so that U was connected with B.

2. f_1 removed from w and then K liberated, so that U was discharged.

3. f_2 pressed against w , so that L was discharged through the unshunted galvanometer, producing a deflection d_2 .

The *rationale* of the process will easily be seen. By making L part of the brass box while charging, we are independent of the action of its edge and of the shape of the curved ribs on its lower surface (see figs. 1 and 3); and by connecting U with the box before discharging L we obtain a complete discharge from the latter.

The experiment was occasionally varied by leaving L insulated for *some time* after putting U to earth; and the apparatus was not considered in good order if any perceptible diminution from leakage was observed in the discharge of L to result from an insulation of several seconds.

Let C be the current, in absolute electromagnetic units (gramme, centimetre, second), which flows in the first case through the galvanometer.

Let a_1 be the angular deflection produced,

g the resistance in absolute units of the galvanometer,

s " " " " " shunt,

R the resistance in absolute units introduced into the circuit,

G the magnetic galvanometer-constant,

H the horizontal intensity of the uniform magnetic field in which the needle moves;

then $C = \frac{H}{G} \tan a_1$ approximately.

Let V be the difference of potentials maintained by the battery at the points A and B; then

$$C = \frac{s}{s+g} \cdot \frac{r_1}{r_1+r_2} \cdot \frac{V}{R + \frac{sg}{s+g}}$$

$$= \frac{r_1}{r_1+r_2} \cdot \frac{sV}{(s+g)R + sg}.$$

Let K be the capacity of the air-condenser in absolute electromagnetic units (gramme, centimetre, second); then if a_2 is the angular swing produced by the discharge in the second case, and P the periodic time in seconds of the needle swinging freely,

$$VK = \frac{HP}{\pi G} \sin \frac{a_2}{2};$$

$$\therefore K = \frac{P}{\pi} \frac{r_1}{r_1+r_2} \frac{g}{(s+g)R + sg} \frac{\sin \frac{a_2}{2}}{\tan a_1}$$

$$= \frac{P}{\pi} \frac{r_1}{r_1+r_2} \frac{s}{(s+g)R + sg} \frac{d_2}{2d_1} \text{ approximately,}$$

a result quite independent of the electromotive force or resistance of the battery.

If a is the area of the lower plate of the condenser in square centimetres,

t the distance in centimetres between the plates,

k the electrostatic capacity in absolute units (gramme, centimetre, second),

$$k = \frac{A}{4\pi t};$$

but

$$k = v^2 K;$$

$$\therefore v = \sqrt{\frac{A}{4\pi t} \frac{\pi}{P} \frac{r_1+r_2}{r_1} \frac{(s+g)R + sg}{s} \frac{2d_1}{d_2}}.$$

d_2 is supposed to be the *undamped* deflection of the galvanometer; but as there was always some slight damping even in our *ballistic* galvanometer, the following correction must be introduced: for d_2 we must write $(1 + \frac{1}{2}\lambda)d_2$, where λ is the logarithm to the base e , or 2.71828, of the decrement.

If now all the resistances be measured in ohms, the complete expression for v becomes

$$v = \sqrt{\frac{A}{4\pi t} \cdot \frac{\pi}{P} 10^9 \frac{r_1 + r_2}{r_1} \cdot \frac{(s+g)R + sg}{s} \frac{2d_1}{(1 + \frac{1}{2}\lambda) \cdot d_2}}.$$

In actual practice, of course, the mean of a large number of discharges of the air-condenser was employed, and great care had to be taken that the needle was absolutely at rest before each discharge—since with such a large moment of inertia an extremely small angular velocity meant a considerable angular momentum, and consequently a considerable error if disregarded. Consequently, even when a number of weak auxiliary checking currents were employed to stop the needle while swinging after a discharge, considerable delay had always to occur between two successive discharges, while waiting for the needle to come to rest.

June 18th.

$$\begin{aligned} A &= 1324.96 \text{ square centimetres,} \\ t &= 1.024 \text{ centimetre,} \\ P &= 25.3 \text{ seconds,} \\ r_1 &= 3.0045 \text{ ohms,} \\ r_1 + r_2 &= 8538 \text{ ohms,} \\ R &= 12000 \text{ ,,} \\ \frac{s}{s+g} &= \frac{1}{1000}, \\ d_1 &= 297.34 \text{ scale-divisions,} \\ d_2 &= 261.63 \text{ ,, ,, mean of 39 discharges,} \\ \frac{1}{2}\lambda &= 0.12095. \end{aligned}$$

Weight of the magnet complete, with the forty magnets, the wooden segmental pieces, and the mirror,

$$\begin{aligned} &2.15 \text{ grammes,} \\ v &= 297.4 \text{ million metres per second.} \end{aligned}$$

June 23rd.

$$\begin{aligned} A &= 1323.14 \text{ square centimetres,} \\ t &= 0.7728 \text{ centimetre,} \\ P &= 39.5 \text{ seconds,} \\ r_1 &= 3.0045 \text{ ohms,} \\ r_1 + r_2 &= 10037.16 \text{ ,,} \\ R &= 12000 \text{ ,,} \\ s &= 19.955 \text{ ,,} \\ g &= 19733 \text{ ,,} \\ d_1 &= 247.75 \text{ scale-divisions,} \\ d_2 &= 221.93 \text{ ,, ,, mean of 41 discharges,} \\ \frac{1}{2}\lambda &= 0.07825. \end{aligned}$$

Weight of the needle complete, with the forty magnets, the small segments cut from leaden sphere, and the mirror,

$$v = \begin{array}{l} 3.4 \text{ grammes,} \\ 299.5 \text{ million metres per second.} \end{array}$$

June 25th.

$$\begin{array}{ll} A = & 1323.14 \text{ square centimetres,} \\ t = & 0.7728 \text{ centimetre,} \\ P = & 42.2 \text{ seconds,} \\ r_1 = & 3.0045 \text{ ohms,} \\ r_1 + r_2 = & 10040 \quad , \\ R = & 12000 \quad , \\ \frac{s}{s+g} = & \frac{1}{1000}, \\ d_1 = & 263 \text{ scale-divisions,} \\ d_2 = & 223.9 \quad , \quad , \quad \text{mean of 18 discharges,} \\ \frac{1}{2}\lambda = & 0.081865. \end{array}$$

Weight of the needle complete, with the forty magnets, the small segments cut from the leaden sphere, and the mirror,

$$v = \begin{array}{l} 3.4 \text{ grammes,} \\ 297.2 \text{ million metres per second.} \end{array}$$

Mean of the three values of v (*i. e.* the final result from the ninety-eight discharges of the air-condenser) is

$$298 \text{ million metres per second,}$$

or exactly the velocity found by M. Foucault for light.

The probable error of our answer (298) is about 1 per cent. Now the difference between M. Foucault's velocity for light (298 million metres per second) and M. Cornu's (300 million metres per second) is less than 1 per cent. We may therefore conclude that these two velocities for light, as well as the value we have obtained by the method which theoretically ought to give the most accurate determination for the ratio of the electromagnetic to the electrostatic unit of quantity, are all equal within the limits of our experiments.

XLV. Notices respecting New Books.

The Study of Rocks, an Elementary Text-book on Petrology. By FRANK RUTLEY, F.G.S. London: Longmans and Co. 1879.

THE study of rocks has of late years assumed considerable interest and importance. Their classification, based on their different origins, modes of occurrence, or mineral character, has been now further supplemented by an examination of the microscopic structure.

In this latter field of inquiry the suggestive paper by Mr. Sorby* in 1858 led the way, which was afterwards followed by the elaborate researches of Zirkel and those of Rosenbusch, Vogelsang, von Lasaulx, Bořický, and other continental petrologists, while in our own country the labours of Allport, Bonney, D. Forbes, J. A.

* "On the Microscopical Structure of Crystals," *Quart. Journ. G. S.* xiv. p. 453.

Phillips, Rutley, and C. Ward have also advanced this special branch of geology. In fact the application of the microscope seems to be an essential element of petrological research, as it has afforded of late years more precise information concerning the mineral constitution and minute structure of rocks, than it was possible to acquire by the older methods of research.

It is true that most manuals of geology contain descriptions and classifications of rocks; but few English works have been specially devoted to their study. Of these Pinkerton, Macculloch, the translations of Cotta and Jannetaz are well known; with these exceptions, comparatively little has been done in this country to supply elementary instruction in the systematic study of rocks, although several good manuals have recently been published on the Continent. To remedy this deficiency, Mr. Rutley has prepared the above textbook for the guidance of students in this branch of science.

Having specially devoted himself to this subject, and being fully conversant with the works of foreign and British authors, he has more or less plentifully interwoven original ideas and observations with the information derived from these sources, in the general treatment of the different subjects.

The work is divided into two parts. The first, or rudiments of petrology, comprises the nature of rocks, their origin and structure, mode of occurrence, the collecting and arrangement of them, the method of preparing sections of rock for microscopic investigation, followed by descriptions of the form, chemical composition, megascopic and microscopic characters of the chief rock-forming minerals, upon the identification of which the determination of the precise character of a rock is necessarily based. The second part, or descriptive petrology, contains the classification of rocks, in which the author to some extent deviates from that commonly adopted. The two principal divisions are the Eruptive and Sedimentary, the former being again divided into two classes—the vitreous and the crystalline. Besides careful descriptions of the various rocks, the author has prepared some suggestive and useful Tables (partly after the manner of Senft), showing a scheme of deviations of certain rocks, as Granite, Trachyte, Diorite, Basalt.

Limited to some extent as to space, which necessarily requires that certain portions of the subject should be treated with brevity, Mr. Rutley has nevertheless produced a very useful manual of petrology, in which the various parts are clearly and concisely described; so that by a careful study of the book, assisted by the examination of the chief rock-forming minerals, and supplemented by some field-work, the student will be enabled to master this somewhat difficult branch of geological science.

Annual Report of the Department of Mines, New South Wales, for the Year 1877. 4to. Pp. 212. Richards, Sydney; Trübner, London, 1878.

A geological map of about 240 square miles of Upper Palæozoic country indicates some of the steps attained by the progress of the Geological Survey of New South Wales; and the many elaborate Reports by the relatively few Surveyors show their industry and

high scientific knowledge. The immediately practical advantages of the science, rather than its theoretical and gradually available results, are mostly aimed at by Colonial Governments, with their new-world notions of forcing a harvest, whether of trade, minerals, politics, or crops, with the impatience of "Teutonic gold-diggers" rather than the unhurried seeking of small profits by the gradually enriched Chinaman.

Hence the best geological intellects in the Colony are directly applied to the determination of gold-fields, the examination of coal-seams, and the proving of minerals, rather than to the slower mapping of soils and strata, and description of the physical geography of the region, whether it be agricultural, pastoral, or mineral, or combining two or more of these characters.

Geology, however, gives good results, in whatever direction its chief aim is temporarily directed; and though hurried from mine to mine, the educated Surveyor observes and notes the conditions and structure of the country traversed, and spreads his knowledge as he goes. The connecting rocks between rich spots, the runs or leads of drifted material rich with gold, the origin of the drift, the lie and direction of wealthy veins, and the nature of ores must all be studied in relation to our knowledge of similar phenomena elsewhere. The merely local observer and the provincial wiseacre are sure to lead their little world astray with crude notions, false conclusions, and insane crochets. Hence the policy and wisdom of the Colonial Government in doing the best they can to ensure their districts being duly geologized as far as circumstances permit.

That an enlightened policy supports the New-South-Wales Survey and gets good results the present and preceding Reports fully show. Though taking specially a local mining aspect they are rich with matter that will help to advance Geology and Mineralogy, both practical and theoretical.

The details of the coal-seams and their produce, and of the gold-works, copper-mines, opal-diggings, &c., are of especial value; and in view of the present scarcity of gold, to which our commercial depression is now said to be largely due, it is agreeable to note that the Government Geologist, Mr. Wilkinson, is sanguine as to new discoveries of Australian gold-fields.

XLVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 216.]

Feb. 5, 1879.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Occurrence of Pebbles with Upper-Ludlow Fossils in the Lower Carboniferous Conglomerates of North Wales." By Aubrey Strahan, Esq., M.A., F.G.S., and Alfred O. Walker, Esq., F.L.S.

The authors described the mode of occurrence near Abergele of certain Lower Carboniferous conglomerates, best exposed in Ffernant

Dingle, and especially of one containing numerous red- and green-sandstone pebbles, which enclose fossils of Upper-Ludlow forms, and lying above the so-called "Bastard Limestone." From the arrangement of the beds the authors believe that they may have been deposited against a bank or sloping surface of Wenlock shale; and they state that the great majority of the pebbles in the conglomerate are quite unlike any rock known in the district, but closely resemble the Upper-Ludlow beds of Kendal and Central Wales. The authors discuss the origin of the pebbles, and suggest "the probable extension of the Ludlow beds under Lancashire as the most likely source from which they can have been derived."

2. "On a New Group of Pre-Cambrian Rocks (the Arvonian) in Pembrokeshire." By Henry Hicks, M.D., F.G.S. With an Appendix on their Microscopic Structure, by T. Davies, Esq., F.G.S.

In some new areas of Pre-Cambrian rocks, discovered by the author last summer in Pembrokeshire, some rocks of a character hitherto unrecognized in this country were made out. As they were found to hold there, and subsequently also in other areas, a very definite stratigraphical position, with a vertical thickness of several thousand feet, they have been separated by the author from the other Pre-Cambrian groups under the distinctive name of Arvonian. They were also found to occupy an intermediate position between the Dimetian and Pebidian formations, and at all points, so far as could be made out, appeared to be separated from each of those formations by stratigraphical breaks. The new areas where they are chiefly exposed are situated some few miles to the north of Haverfordwest, where they form ridges running in a direction from N.E. to S.W. They occupy an average width of about a mile, attain at some points to a height of nearly 600 feet, and together have a length of over nine miles. The rocks are flanked by Pebidian and Cambrian beds along their N.W. borders; and on the S.E. Silurian rocks have been brought against them by faults. In general appearance, as well as in their more minute lithological characters, they are easily distinguished from any of the rocks hitherto described by the author as characteristic of the Dimetian and Pebidian groups in Pembrokeshire. They are, however, so closely allied to some of the true "*hällfrinta*" rocks of Sweden, that it seems to the author and Mr. Davies that this is the name that should be applied to them in a petrological sense. In external aspect and in their splintery fracture they resemble a hornstone. Under the microscope they are seen to consist mainly of a crypto-crystalline ground-mass, which, when examined with a high objective, is resolved into grains of quartz, with an interstitial ingredient having but little action on polarized light, but which presumably is felsite. There are also numerous nests and fissure-like groups of quartz-grains disseminated throughout; and sometimes angular fragments, distinct in size and shape, are enclosed. These nests and fissure-like groupings are frequently encircled also with bands of a fibrous chalcedony, the structure of which is well exhibited with polarized light; and a rude parallelism, suggestive either of an incipient foliation or of

stratification, is thereby given to the rock. The author and Mr. Davies believe the origin of the rock to have been a sedimentary one.

3. "On the Pre-Cambrian (Dimetian, Arvonian, and Pebidian) Rocks of Caernarvonshire and Anglesey." By Henry Hicks, M.D., F.G.S. With an Appendix on their Microscopic Structure, by the Rev. Prof. T. G. Bonney, M.A., F.R.S., F.G.S.

In this paper the author gave the results of some further researches made in Caernarvonshire and Anglesey since his previous communication to the Society on Dec. 5, 1877. A brief statement of some of the results was read at the last meeting of the British Association in Dublin; but much additional evidence was now brought forward, besides many important facts obtained since by microscopical examination of the rocks. Concerning the areas described in his former paper much additional information was given, and the boundary in one case greatly extended. This new area lies to the west of Moel Tryfaen, and includes now, in addition to the central or quartz-felsite ridge, the whole of the rocks marked in the Survey maps as altered Cambrian, extending as far west as Glynllifan. Many of the large masses in South-west Caernarvonshire and the Llyn promontory, hitherto supposed to be intrusive rocks of Silurian or Post-Silurian age, were discovered, during these researches, to be of Pre-Cambrian age, and conclusive evidence obtained that the so-called altered Cambrian rocks there, and in Anglesey, were also of that age. In these various areas the three Pre-Cambrian formations found in Pembrokeshire were recognized by having similar lithological characters, and by holding almost identical stratigraphical positions in their relations to one another. *Dimetian* rocks were recognized at Twt Hill, Rhos Hirwani, near Ffestiniog, and in the so-called granitic ridge in Anglesey; *Arvonian* rocks between Caernarvon and Menai Bridge, in the Eifl Range, Nevin Mountain, and near Ty Croes in Anglesey, &c. &c.; *Pebidian* rocks to the east of Glynllifan, Bangor, at the lower part of the Llyn promontory, and in many places in Anglesey. Some notes on the section near Ty Croes by Prof. Bonney accompanied the paper, in addition to an appendix by him on the microscopic examination of rock specimens from each of the areas examined.

4. "On the Quartz-felsite and Associated Rocks at the base of the Cambrian Series in North-western Caernarvonshire." By the Rev. Prof. T. G. Bonney, M.A., F.R.S., F.G.S.

The great masses of quartz-felsite (or quartz-porphyry) which occur in the vicinity of Bangor, Caernarvon, and Llyn Padarn are coloured in the Survey map as intrusive, and in the memoir regarded as most probably the result of an extreme metamorphosis of the lower beds of the Cambrian series.

The author showed that these quartz-felsites exhibited, in places, all the characteristics of true igneous rocks—flow-structure, fissile structure, and the more ordinary structure of rhyolitic rocks; that they were, in one place, at least, associated with masses of agglomerate, and in another parted by a band of comparatively unaltered slate.

He also showed that in several places there succeeded a grit formed of fragments of it, that larger fragments of perfectly

characteristic structure, associated with others of a more slaggy and scoriaceous type, occurred repeatedly in the overlying beds up to the base of the Cambrian, described by Prof. Hughes and Dr. Hicks, the felsite pebbles in which come from the same source.

Lastly, he showed that the signs of metamorphism and apparent "melting down" asserted to be visible on the sides of Llyn Padarn, proved, on microscopic examination, to be mainly superficial. Hence he maintained that these rocks were rhyolitic lava-flows of Pre-Cambrian age.

5. "On the Metamorphic Series between Twt Hill, Caernarvon, and Port Dinorwic." By the Rev. Prof. T. G. Bonney, M.A., F.R.S., F.G.S., and F. T. S. Houghton, Esq., B.A.

In the Geological-Survey map this district is coloured as "intrusive felsite," together with those spoken of in the last paper. It was asserted to be probably metamorphic rock by Prof. Hughes and Dr. Hicks in a communication made to the Society last year; and the first author confirmed that view by microscopic examination of a specimen collected by them. The authors had during the past autumn more minutely examined the district, and found:—(1) that the general character of the series was that of a metamorphic one; (2) that the rocks of granitoid aspect were associated with well-marked beds of conglomerate; (3) that this series extended up to a little beyond Port Dinorwic, where the quartz-felsite set in. The paper described the microscopic structure of some of the rocks; and the author expressed the opinion that the more granitoid specimens were probably the results of alterations of felspathic grits.

Feb. 21.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. A copy of a Letter from the late Acting Governor of the Falkland Islands, relating to the overflow of a peat-bog near Port Stanley, in East Falkland.

2. "Note on *Poikilopleuron Bucklandi*, of Eudes Deslongchamps (père), identifying it with *Megalosaurus Bucklandi*." By J. W. Hulke, Esq., F.R.S., F.G.S.

3. "Note on a Femur and a Humerus of a small Mammal from the Stonesfield Slate." By H. G. Seeley, Esq., F.L.S., F.G.S., Professor of Geography in King's College, London.

4. "A Review of the British Carboniferous Fenestellidæ." By G. W. Shrubsole, Esq., F.G.S.

March 12.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On Perlitic and Spherulitic Structures in the Lavas of the Glyder Fawr, North Wales." By Frank Rutley, Esq., F.G.S.

The rock, to the eye and under the microscope, has all the appearance of a felstone, but under the latter also exhibits perlitic structure as clearly as one of the Saxony perlites. Some of the other felstones of the Glyder Fawr show numerous spherulites. These felstones have been determined by the Survey to be lavas of Bala age.

2. "The Gold-leads of Nova Scotia." By Henry S. Poole, Esq., M.A., F.G.S., Government Inspector of Mines.

The author remarked upon the peculiarity that the gold-leads of Nova Scotia are generally conformable with the beds in which they occur, whence Dr. Sterry Hunt and others have come to the conclusion that these auriferous quartz veins are interstratified with the argillaceous rocks of the district. With this view he does not agree. He classified the leads in these groups according to their relations to the containing rocks, and detailed the results of mining-experience in the district, as showing the leads to be true veins by the following characters:—1. Irregularity of planes of contact between slate and quartz; 2. The crushed state of the slate on some foot-walls; 3. Irregularity of mineral contents; 4. The termination of the leads; 5. The effects of contemporary dislocations; 6. The influence of strings and offshoots on the richness of leads. The author further treated of the relative age of the leads and granite, and combated the view that the granites are of metamorphic origin, which he stated to be disproved by a study of the lines of contact. He also noticed the effects of glaciation on the leads, and the occurrence of gold in Carboniferous conglomerate.

3. "On Conodonts from the Chazy and Cincinnati groups of the Cambro-Silurian, and from the Hamilton and Genesee-Shale divisions of the Devonian, in Canada and the United States." By G. Jennings Hinde, Esq., F.G.S.

4. "On Annelid Jaws from the Cambro-Silurian, Silurian, and Devonian Formations in Canada, and from the Lower Carboniferous in Scotland." By G. Jennings Hinde, Esq., F.G.S.

XLVII. *Intelligence and Miscellaneous Articles.*

ON THE DIFFUSION OF LIQUIDS. BY J. STEFAN.

THIS memoir contains the calculation of Graham's experiments, which were published in the 'Philosophical Transactions,' 1861, p. 183. To commence the experiment, on the top of 100 cub. centims. of a salt-solution in a cylindrical vessel 700 cub. centims. of water was poured. After a fixed time the liquid was, by means of a fine siphon, drawn off at the top in portions of 50 cub. centims. each, and the salt-content of each of the fourteen upper layers separately, the two lowermost together, determined. The amount of salt contained in the original solution was always 10 grams.

The solution, corresponding to this arrangement of the experiment, of the differential equation constructed by Fourier for calculating the propagation of heat through conductors, and applied by Fick to the representation of the laws of diffusion, can be effected in two ways:—First, in the form of periodic series. This form is ill-suited to the discussion of the experiments, since, except in a few cases, very many terms of the series must be taken for the calculation.

The second method of solution is used in the form of definite integrals; and for them proper Tables are calculated. The heads

of these Tables contain the salt-contents of the respective layers in the diffusion-vessel for a series of values of a number dependent on the coefficient and the duration of the diffusion. Given these two quantities, the corresponding distribution of the salt is found in the Tables.

Conversely, with the aid of these Tables, the value of the diffusion-coefficient belonging to each salt-content of a layer as given by Graham can be found, and we can recognize, according to the nature of the accordance or deviation of the coefficients resulting from the data of one and the same experiment, whether the experiment is in harmony with the theory or not.

Diffusion-processes are very easily disturbed by currents produced in the liquid by differences of temperature, so that the upper layers receive too much, and the deeper layers too little salt. The results of Graham's experiments, however, may be more faulty still, and in the same direction, in consequence of those currents which can hardly be avoided when the liquid is drawn out with the siphon. We must therefore, even assuming that the theory accurately represents the processes of diffusion, be prepared to expect considerable differences between observation and calculation. Such differences occur in many cases; but there are also cases in which the deviations are very slight. The following series give instances of the close approximation that may exist between observation and calculation:—

Layer.	I.		II.		III.	
	O	Calc.	Obs.	Calc.	Obs.	Calc.
1. }	3.284	3.328	5.392	5.391	2.936	3.004
2. }						
3.	1.527	1.482	1.930	1.928	1.387	1.369
4.	1.317	1.290	1.282	1.287	1.236	1.225
5.	1.057	1.073	0.727	0.751	1.070	1.056
6.	0.850	0.853	0.376	0.380	0.876	0.877
7.	0.640	0.648	0.170	0.167	0.700	0.704
8.	0.460	0.469	0.071	0.064	0.542	0.541
9.	0.318	0.325	0.024	0.021	0.403	0.402
10.	0.211	0.215	0.011	0.006	0.289	0.288
11.	0.134	0.135	0.005	0.002	0.204	0.199
12.	0.081	0.082	0.003	0.001	0.135	0.133
13.	0.051	0.047	0.002		0.092	0.086
14.	0.028	0.026	0.002		0.058	0.055
15.	0.017	0.015	0.002		0.040	0.036
16.	0.013	0.011	0.001		0.032	0.028

I. refers to an experiment with chloride of sodium, lasting seven days, at the temperature of 9° C.; II. to an experiment with cane-sugar, of six days' duration, at 9°; III. to an experiment with a mixture of the chlorides of potassium and sodium in equal parts, of seven days' duration, and at 11–12° temperature.

This Table shows that the mathematical treatment applied to the diffusion-processes gives their course with very close approximation.

The motion of diffusion has two properties in common with wave-motion, which follow from the linear form of the differential equations determining the laws of these motions. The first is the superposition of the diffusion-currents which start from different parts of the liquid; the second is the complete reflection which the diffusion-currents undergo at the boundaries of the liquid. Both properties can with great advantage be made use of in the calculation of Tables. The memoir contains also a formula, based on them, which permits the diffusion-coefficient to be calculated in a very simple way from a combination of the salt-contents of the individual layers.

As regards the amount of these coefficients k , *inter alia* there were found :—

For Caramel (temp. 10°)	$k=0.047$
Albumen (13°)	$k=0.063$
Cane-sugar (9°)	$k=0.312$
Chloride of sodium (5°) ..	$k=0.765$
(9°) ..	$k=0.910$
Hydrochloric acid (5°)	$k=1.742$

and these numbers have for base the centimetre as unit of length, and the day as unit of time.

In relation to the diffusion of quantities of salt, older experiments of Graham, and Marignac's comprehensive experiments especially, have taught us that the parts of the mixture essentially effect one another, so that the more diffusible of the two salts in the mixture diffuses still more quickly, and the other still more slowly, than when it alone is present. Also, from the experiments here discussed it follows, at the same time, that the distribution of each of the two salts, especially of the more quickly diffusing one, sensibly deviates from the laws of simple diffusion. So much more remarkable is it that the distribution of the mixture as a whole, as shown by the series under III., so closely conforms to those laws.

There are two experiments of this kind specially important. The first refers to a mixture of chloride of potassium and sulphate of soda, the second to a mixture of chloride of sodium and sulphate of potass. Both proceed in almost the same way; and although Graham did not complete the analyses, it may yet be concluded from his statements that the upper layers contained chloride of potassium in the second experiment also.

Such cases of decomposition by diffusion, as Graham calls them, were already known from his older experiments. It is more correct to assume that the decompositions take place in the mixture before the diffusion, and that the latter only acts the part of a sieve which lets through more readily the one product than the other.—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe*, 1879, No. 3, pp. 24–27.

ON THE SPECTRUM OF OXYGEN, AND ON THE ELECTRICAL LUMINOUS PHENOMENA OF RAREFIED GASES IN TUBES WITH LIQUID ELECTRODES. BY M. PAALZOW.

As a rule, in the examination of the electrical luminous phenomena.
Phil. Mag. S. 5. Vol. 7. No. 43. April 1879. 2 A

mena of rarefied gases, the electricity is conducted to the gas enclosed in a glass tube through metal wires fused into the tube. Since the metals are very likely to exert an influence upon the phenomena, I have tried the insertion of a liquid between the metal and the gas, in order, to some extent, to form liquid electrodes.

A glass tube twice bent at right angles contained in its wider parts fused-in platinum wires and concentrated sulphuric acid, the latter rising one centimetre above the wires. The tube was joined by fusion to a mercury air-pump, the drying-vessel of which was filled with solid phosphoric acid.

The gases contained in the tube and the pump having been so far rarefied that an induction-current could pass through, the platinum wires were connected with the poles of a Ruhmkorff inductorium, which, excited by four Bunsens, gave a striking-distance of 71 millims. and a deflection of 50 scale-divisions on a mirror-compass. (A constant current of 0.00035 Siemens-Daniell unit gave, on the same compass, with the same spirals, a deflection of 100 scale-divisions. The value of the 50 scale-divisions of the momentary current, calculated from the duration of a vibration of the damped magnet and from its logarithmic decrement, amounted to 0.000013 S.-D. unit.)

The luminous phenomena observed in the tube under these circumstances are in general similar to those seen in tubes the wires of which are provided with metal disks.

The positive light starts from the bounding line of the surface of the liquid and the glass wall, and spreads, in narrower or wider strata (according to the strength of the pressure of the gas), to the vicinity of the negative liquid.

From the surface of the negative liquid itself there rises, at some distance from it, a slightly conic ring of light, similarly to the flame of a ring-shaped burner. The intensity of this ring diminishes from below upwards. The more the rarefaction increases, the more does this negative luminous cylinder lengthen, and the greater becomes its distance from the surface of the liquid. With the strongest rarefaction the luminous phenomena are almost the same at both poles. The negative light makes its appearance also in the narrow parts of the tube*. When the entire tube is inclined so that the liquid surfaces are bounded by ellipses, the positive light emanates from the highest part of the boundary, the negative is most intense at the lowest point, but the entire ring of light remains parallel to the sides of the tube.

The magnetic deflection of the positive and the negative light is the same as that in the before-mentioned tubes with metal disks.

The whole tube is besides filled with diffused glittering light, which when nitrogen is present is greenish (according to Morren proceeding from the formation or decomposition of the compound $\text{NO}_3 + 2\text{SO}_3$); without nitrogen it is bluish, and then perhaps arises from the vapour of sulphuric acid. This light can be insulated; and then it gives a continuous spectrum.

The entire luminous process is accompanied by decomposition of

* Goldstein, *Berl. Monatsber.* May 1876, p. 279.

sulphuric acid ; from all parts of the immersed platinum wires gas-bubbles are seen to rise. The positive wire furnishes oxygen, the positive liquid surface hydrogen ; and it is the reverse at the negative side. Various other examples can be cited in proof that a dividing surface between a liquid and a gas may be regarded as an electrode. This view receives especial confirmation from the phenomena of occlusion *. If, namely, the current has passed through for a long time, *one* induction-shock in the same direction furnishes distinct gas-bubbles ; if now the current be reversed, 7-9 induction-shocks will be necessary at the electrode charged with oxygen before the gas-bubbles are observed, while at that charged with hydrogen certainly 15-17 will be required.

When the electrolytic process was continued during a whole week, predominantly oxygen only was obtained in the tube. Primarily, however, both oxygen and hydrogen are always separated ; but the latter finally precipitates sulphur from the sulphuric acid, which at first makes the liquid thick, and then settles to the bottom.

In order to decipher and explain the complex spectrum, I was obliged to carry out two new operations—to produce a pure oxygen-spectrum, and to determine the conductivity of the pure gases oxygen, hydrogen, nitrogen. I permit myself here only to report upon the oxygen-spectrum, reserving for subsequent communications the conductivity of the gases mentioned.

In the tube there might be contained nitrogen, hydrogen, oxygen, mercury-vapour. On the spectrum of oxygen the most diverse results have been given by Plücker (*Pogg. Ann.* cvii. p. 497), Wüllner (*Pogg. Ann.* cxxxv. p. 377), Salet (*Ann. d. Chim.* [4] xxviii. p. 5), Vogel (*Pogg. Ann.* cxlvi. p. 569), Huggins (*Phil. Trans.* cliv. p. 139), Plücker and Hittorff (*Phil. Trans.* clv. p. 1), and Schuster (*Proc. Roy. Soc.* xxvii. p. 383).

The tubes for examination I filled with oxygen :—(1) after the method previously described, by decomposition of the concentrated sulphuric acid by the induction-current, when finally oxygen was almost solely evolved ; (2) by fusing to the experiment-tube a voltameter containing concentrated sulphuric acid, six Bunsen cells being employed for the decomposition ; (3) by heating chlorate of potass in a retort united directly to the tube by fusion.

I was at length obliged to abandon the use of a gasometer and any other drying-apparatus than that attached to the air-pump, containing solid phosphoric acid, since they never furnished pure results.

Tightening the stoppers and cocks with tallow, caoutchouc, or concentrated sulphuric acid gave the same results.

The filling and exhaustion of the vessels and pump were of course repeated until the phenomena became constant—most frequently from 40 to 50 times †.

I have always found only one oxygen-spectrum, consisting of

* Helmholtz, *Pogg. Ann.* cl. p. 483.

† I made use of the appearance of the intensely green fluorescence-light of glass as a sign that the exhaustion of the tubes was as complete as possible.

five bright lines *. I determined their positions by means of a Steinheil's apparatus with a prism, according to a scale, in which

	C	D	E	b	F	G
are situated at . . .	35	50	69.5	74	87	117

If the five lines from the red onwards be designated by O_α , O_β , O_γ , O_δ , O_ϵ , we have

	O_α	O_β	O_γ	O_δ	O_ϵ
at	45	57.5	72	88.5	158

With the aid of the spectral apparatus with four prisms lent me by M. Kirchhoff, I determined the lines according to Kirchhoff's plate, according to which

	O_α	O_β	O_γ	O_δ	O_ϵ
are situated at	935	1231	1625	2164	2489

From comparison of these Kirchhoff's lines with Ångström's plates the wave-lengths of

	O_α	O_β	O_γ	O_δ	O_ϵ
are	602	558.2	519	481	453

The intensity of O_γ is the greatest; then follow O_β and O_δ , and last O_ϵ and O_α . The lines are sharp towards the red end of the spectrum, fading towards the violet. Their wave-lengths agree best with those given by M. Vogel; only he did not observe O_α , perhaps because the pressure of the gas was not sufficient. Plücker's lines O_α and O_δ are probably mercury-lines.

Contrary to Mr. Schuster's latest statement, I find the spectrum of pure oxygen equally at both poles. I find this identity in the hydrogen also, and in the nitrogen only an intensifying of two of its lines (95 and 125 of the first scale). If the gases are not pure, other lines may come in at the negative pole, because the ponderable mass is thrown off at that pole.

I have intentionally employed only simple induction-currents, because with the rapid and violent discharge of a Leyden jar portions of the electrodes and glass sides may be carried along in the discharge-current, which with the quiet discharge of the simple induction-current remain undisturbed in their places. At all events I hold that the question whether there is a plurality of spectra of a pure gas is still an open one, and am inclined rather to ascribe to each simple gas *one* spectrum only. With oxygen, which I have pursued within variations of pressure from 200 millims. to the most extreme rarefaction, I have never seen any other lines than the five named; and these, brightest at 2 millims. pressure, diminish in brightness in both directions from that point, so that at very inconsiderable and at high pressures only a glimmer of light is visible, which I would not call a continuous, but much rather an indistinct spectrum.—*Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*, Sept. & Oct. 1878, pp. 705-709.

* Between O_β and O_γ three, before O_α four, and behind O_ϵ one broad streak of light are to be seen, but so faint that they can never be confounded with the five lines; besides, they are quite destitute of a sharp margin, and cannot be analyzed with the four-prism apparatus.

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[FIFTH SERIES.]

MAY 1879.

XLVIII. *On the Formation of Emulsions, and the Action of the Bile in Digestion.* By Dr. G. QUINCKE*.

1. *Historical Review.*

AN emulsion consists of a large number of small spherical globules of fatty matter suspended in an aqueous liquid. Ordinary milk, for instance, is an emulsion. The smaller these fatty globules the larger is their surface in proportion to the mass, and the greater is the resistance they meet with in ascending through the specifically heavier surrounding fluid. The smaller the fatty globules the longer they remain suspended in the surrounding liquid, and the more perfect is the emulsion. The minute globules have a continual tendency to coalesce into larger ones; the less this tendency and the smaller the uniform velocity of the ascent of the globules in the surrounding aqueous liquid, the more permanent is the emulsion. And the smaller this velocity the less is the difference between the specific gravity of the emulsion and that of an actual solution of the fatty matter in the liquid†.

In chemists' shops an emulsion is made by diffusing mechanically (rubbing with a pestle in a porcelain mortar) the globules of an oil throughout a solution of gum arabic in water. In the process of digestion in the animal body the assimilation of the fats is initiated by the formation of an emulsion in the liquid contents of the intestine, and, as experience shows, faci-

* From a separate impression from Pflüger's *Archiv für die gesammte Physiologie*. Translated by J. F. Iselin, M.A.

† Camillo Bondy, Pogg. *Ann.* 1865, p. 323; E. Mach, *ib.* p. 329.
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litated by the bile. It is, on this account, as a physiological question that the development and durability of emulsions has more especially been studied.

W. Kühne (*Physiologische Chemie*, p. 129, 1866) and Brücke (*Wien. Sitzber.* 1870, lxi. 2nd part, p. 363) were among the first to demonstrate the influence of the soaps developed in the small intestine on the formation of emulsions. The latter showed that rancid oil, containing free fatty acid, when agitated with dilute solution of the alkaline carbonates, produces a perfect emulsion with much greater rapidity than a neutral oil. He pointed out at the same time the important part played in the process of digestion by the free fatty acids, which, according to the discovery of Claude Bernard, are separated in the intestinal canal from the neutral fat by means of the pancreatic juice. More recently Johannes Gad* made the interesting observation that small quantities of oil, which contain free fatty acids, will form perfect emulsions by mere contact with alkaline solutions without the aid of external or mechanical means, such as agitating or stirring. This can be well shown by dropping a little cod-liver oil into a 25-per-cent. solution of soda.

From further investigation it appeared that the emulsifying power depends on the viscosity and acidity of the oil, on the concentration of the soda solution, and on the solubility in the surrounding fluid of the soap formed from the fatty acid. By adding common salt and bile to the alkaline liquid, this tendency to dissolve is so far corrected that the facilities for the production of a good emulsion are much increased. With castor-oil, which is more viscous than other oils, the formation of an emulsion was not observed. When the conditions for the emulsion-formation were present, the surface of the drop of oil threw off at once a milky substance into the surrounding liquid; the drop formed protuberances at the side, and exhibited alterations in form and movements which possess remarkable similarity to those of the *Amœba*. Smaller oil-globules then split off; and these partly gave rise to the further production of emulsion. Under the microscope with a low power the vicinity of the drop was seen to be the scene of brisk action; the particles producing turbidity in the fluid were observed to fly off in rapid gyrations from the surface of the drop, sometimes returning to it again. The remainder of the oil could not by mechanical means be converted into emulsion in the same liquid.

Gad recognizes therefore, quite correctly, the conditions

* Du Bois-Reymond's *Archiv für Anatomie und Physiologie*, 1878, pp. 181-205.

for emulsion-development, not in the soap dissolved in the liquid, but in that which forms more or less rapidly on the surface of the oil, where, in fact, the fatty acids contained in the oil come into contact with the alkaline liquid. He then proceeds to observe that when the soap that has been formed under the given conditions is soluble in the surrounding liquid, it is by diffusion carried off radially from the point where it is developed, while inside the globule the equilibrium of solubility is maintained by diffusion of the fatty acid outwards towards the periphery. If the soap be so rapidly formed and removed that the fatty acid by diffusion towards the periphery cannot make up the deficiency, the outer edge of the globule will alter its shape, and there will be thrown off smaller fatty particles which are not enclosed in a soapy membrane. But when the rapidity with which the soap is formed reaches a certain point, the latter will not be dissolved by the surrounding liquid, and the oil-globule will be enveloped by a soapy membrane. The development and displacement of the soapy matter will give rise to a change of volume in the liquid; and this in its turn will cause the breaking-away of small fatty particles, each of which is enclosed in a film of soap. Should there be from any cause an irregularity in the course of the formation of the soapy membranes, it will occasion *Amæba*-like movements, the protuberances being extended at the points where the membrane takes a longer time to thicken.

To these theoretical views, however, I am unable to assent, because it appears to me to be highly improbable that mere diffusion currents can produce movements so energetic as those that are observed in the production of emulsions. In the following pages I shall endeavour to prove that the formation of an emulsion depends essentially on the existence of thin scales of soap solution dispersed over the common surface of the oil and the liquid, and also that the so-called *Amæba*-like movements depend on the same cause.

2. *The Bounding Surfaces of Liquids in Contact with Air and with Water.*

The surface of every liquid, whether the same be bounded by air or by another liquid, has a tendency to become as small as possible, or, as it is commonly termed, has a certain tension. The magnitude of this tension, which may be likened to that of a cloth, or to that of the envelope of a caoutchouc balloon, or of an inflated pig's bladder, is measured by the force (in milligrammes) exerted on a strip of the surface one millimetre broad. The tension of the surface of a liquid bounded by air is at the same time a measure of the cohesion

of the liquid, and is generally found by multiplying together the height at which the fluid will stand in a capillary glass tube, the radius of that tube, and half the specific gravity of the fluid.

In consequence, however, of the difficulty of thoroughly moistening the interior of the tube with the fluid*, this method is not quite accurate. It is a better way to determine the tension by measuring the vertical distance between the apse and vertex of a large flat bubble of air lying in the fluid underneath a horizontal glass plate; the square of this distance into half the specific gravity of the fluid gives the surface-tension directly, independent of the nature of the plate under which the bubble lies. The shape of such a bubble is the same as that of an inverted dewdrop in air. By forming a flat bubble of some other fluid in the heavier fluid (for example a bubble of oil in water) the surface-tension of the common surface of the two fluids may be found in a similar way—for instance, in this case by multiplying the square of the vertical distance between the bubble's apse and vertex into half the difference of the specific gravities of oil and water.

We find by this method the tension at the common surface of air and water to be = 8.25 mgr., of air and olive oil = 3.76 mgr., of water and olive-oil = 2.30 mgr. Other fatty oils, rape-oil, almond-oil, castor-oil, cod-liver oil, &c., give similar results. Fluids which mix with water in all proportions, like alcohol or dilute salt solutions, form neither bubble nor globe in water; the tension of the common surfaces of such fluids and water is = 0†.

3. *The Dispersion of Soap Solutions and of other Fluids over the common Surface of Oil and Water.*

When some other fluid is applied to an air-bubble in water, and it disperses itself over the surface of the bubble, the vertical distance between the vertex and apse of the latter is diminished; in other words, the tension at the common surface of air and water has been reduced. For instance, when olive oil is dispersed over the common surface of air and water, the air-bubble is coated with a thin film of oil; and the surface-tension is now compounded of the tension at the surface of air and oil, and of the tension at the surface of oil and water.

* Compare G. Quincke on the Cohesion of the Solutions of the Salts, Poggendorff's *Annalen*, clx. p. 369, 1877.

† The more detailed explanation of these physical laws, as well as of the theory of the dispersion of one fluid substance over the surface of another, I have given with mathematical and experimental illustrations in Poggendorff's *Annalen*, cxxxix. p. 1, 1870; *ib.* clx. pp. 337 and 560, 1877; and in Wiedemann's *Annalen*, ii. p. 144, 1877.

In a similar way a fluid like soap-solution will spread itself over the surface of a flat oil-bubble in water. At the common surface of soap-solution and olive-oil the tension is $=0.36$ mgr.; but at the common surface of soap-solution and water the tension is $=0$; and as the original surface-tension of olive-oil and water is 2.3 mgr., this tension has been reduced to the extent of 84 per cent. by the dispersion of the solution of soap. It will be found that the oil-bubble itself has been made considerably flatter and broader by the dispersion.

The alteration in form of an air- or oil-bubble increases with the thickness of the film of the applied fluid, and attains its maximum value when that thickness is more than $.0001$ millim., or one-fifth of the length of a mean light-wave in air. The thickness of such a film therefore cannot be recognized by even our best microscopes; for they only detect a distance equal to half a wave-length*. As exceedingly dilute solutions of soap (1 per cent. and less) are able to produce this effect, we observe that an excessively small quantity of solid soap, which in any other way we could scarcely detect, is sufficient to cause this phenomenon of dispersion. And this dispersion takes place with great rapidity; a drop of olive-oil will spread itself over the surface of still water in a single second, and cover a space several metres in diameter.

Similar to the action of soap-solution is that of diluted ox-gall, or of a fluid obtained from the action of a dilute solution of soda on the free fatty acid contained in oil. In a rectangular glass trough a horizontal plane glass plate was suspended, the latter being pierced in the centre with a hole of 2 millims. diameter. The trough was then filled with water up to the lower surface of the glass plate; and through the hole, by means of a pipette, a globule of oil was introduced underneath the glass plate. With a second pipette a few drops of a $.25$ -per-cent. solution of soda were introduced; and this immediately sank in the oil and retreated to the lowest side of the globule. The soda formed with the free fatty acid a soap, which then enveloped the drops of soda solution lying in the oil with a more or less thick whitish membrane. If this membrane be then broken up by agitation, or by its coming into contact with and dissolving in the adjacent water, the solution of soap or a mixed solution of soap and soda spreads itself over the surface of the globule of oil, and the latter becomes flatter and broader.

* The proof of these propositions will be found in my treatise "On the Distance at which the Molecular Force of Capillarity can act," Poggen-dorff's *Annalen*, cxxxvii. p. 402, 1869; also "On the Edge-angle and Dispersion of Fluids on Solid Bodies," Wiedemann's *Annalen*, ii. p. 177, 1877 (Phil. Mag. [5] vol. v. pp. 321, 415).

Simultaneously with this dispersion of the soap-solution, a current is set up from the interior of the fluid towards the centre of dispersion, and continued onwards from its surface. This current is much stronger in the tenacious oil than in the water. The bubble becomes for a short time concave at its vertex; isolated particles of oil are torn away by the current from the main body, and form spherical globules in the surrounding aqueous fluid; and vortices are produced in the oil similar to those caused by blowing a stream of air through a vertical and narrow glass tube on to a free and plane surface of oil. If a layer of oil of from 5 to 10 millimetres thickness be poured on water, and alcohol allowed to spread itself over either the upper or under surface of the oil, those parts of the oil which are opposite to the centre of dispersion will move towards that centre; the layer of oil may even be pierced by this means, so that air and water come into contact. Excessive rapidity of dispersion in the applied fluid, or too much tenacity in the oil, will impede the expulsion of the oil particles or the piercing of the oil layer just as much as sluggishness in the dispersion or too great ductility in the oil.

4. *Explanation of the Spontaneous Formation of Emulsions by means of Dispersion.*

By the phenomena described in the above paragraphs we are now able to explain the formation of an emulsion.

When a solution of soda and an oil come into contact, a solid soap is formed by the action of the free fatty acid of the oil. Gradually a portion of this soap dissolves in the adjoining aqueous fluid. So soon as the fluid solution of soap comes into contact with the oil, it spreads itself over the bounding surface of the oil and aqueous fluid, and carries with it the undissolved particles of soap with any adhering oil-globules. In this way there are detached from the oil and borne into the adjoining fluid a number of filaments, which, possessing the tendency to assume the shape of bodies of least surface, are converted into larger or smaller globules of a spherical form, just as a jet of water in the air breaks up into larger and smaller drops. To a certain extent this conversion of the oil into globules will be retarded by the solid and fluid soap already present or newly formed; and then the length of the filaments will be increased, or the size of the nascent globules diminished. By the original dispersion, however, fresh oil particles will be brought into contact with the soda solution, and after a time the newly formed solid soap is again dissolved and a second dispersion occurs. Similar periodic dispersions of oil on the

surface of still water were observed and described by me nine years ago (Poggendorff's *Annalen*, cxxxix. p. 76, 1870).

The periodic dispersions of soap solution over the common surface of the oil and an aqueous fluid, which are not simultaneous at all points of the oil-surface, will be found to be connected with vortex-movements in the interior of both fluids, and to draw the oil more especially towards the dispersion-centre. This is the cause of the *Amœba*-like movements at the edge of the main mass, while the detached particles of oil form the emulsion. In fact, under the microscope, not only the aqueous fluid can be seen, but also, and more conspicuously, the oil in the vicinity of the common surface, in a state of gyration.

When the oil possesses a tolerable degree of toughness, and the dispersive force at the common surface of the two fluids is moderately great, the vortex-movements and the detached oil particles will be very numerous, and may be produced, as indeed follows from the dispersion theory (see above, 3, p. 305), by means of films of diluted soap solution not exceeding in thickness a few millionths of a millimetre. A very minute quantity of soapy matter is therefore sufficient to produce the appearances in question. The free fatty acid necessary for the formation of this soap is nearly always present in oil, and reaches the surface of the latter by means of diffusion. It can be produced in the fluid contents of the intestine by means of the pancreatic juice, and in the open air by the action of carbonic acid on the neutral alkaline oleates*. If the soap be formed too rapidly, the common surface of the oil and the aqueous fluid is coated with a film of solid soap. Hence the oil-surface becomes immobile, and the dispersion and its consequences are impeded or are altogether wanting—just as, in Leidenfrost's experiment, when water is brought into contact with red-hot metal the formation of steam is impeded or does not occur at all.

In the same way the consequences of the dispersion, the formation of emulsion and the movements of the particles of oil, will be prevented, if only a very small quantity of soap be formed, or if the soap be dissolved too rapidly by the surrounding fluid. Every, even the minutest, particle of soap will then immediately be diffused in a solution, and the dispersive energy will not be sufficient to produce the vortex-movements in the oil, and to detach the particles of that substance. If a mill-brook be allowed to trickle in a small stream over the water-wheel, the latter will not be set in motion; but by periodically damming it back a small body of water can be made with advantage to produce mechanical work.

* Compare Heintz, *Zoochemie*, p. 439.

5. *Solid and Liquid Films at the common Surface of two Fluids.*
Castor-oil.

For the reason last stated, castor-oil will not freely form an emulsion.

When a drop of a fixed oil is allowed to fall at as small a distance as possible from the end of a glass rod onto a layer of a dilute solution of soda which is only a few millimetres in thickness, and is contained in a watch-glass of from 50 to 100 millimetres diameter, the oil will generally, as explained more fully above (in 3), disperse over the surface of the liquid; for the surface-tension of the more perfect fluid is greater than that of the oil. Shortly afterwards soap is formed and dissolved in the aqueous liquid, the tension of the free surface of the aqueous liquid becomes considerably less, and the oil contracts again into a lenticular-shaped drop. On its lower surface the drop of oil is coated with a film of soap, which may consist of a thin membrane either of liquid solution of soap or of solid soap. The latter is well seen when a number of solid particles of soap lying close to each other form a whitish matted film, as in the case of cod-liver oil in a from $\frac{1}{4}$ to 2-per-cent. solution of soda.

If the soapy membrane be liquid, the oil-drop assumes a spherical form, which it will retain even when the liquid in the watch-glass has a rocking or rolling motion communicated to it. If, on the other hand, the membrane be solid, the drop under the rocking motion takes a cylindrical shape, which disappears more or less slowly in proportion as the membrane is thicker or thinner. This was the case with olive, almond, and cod-liver oils in a 0.06-per-cent. solution of soda, and of almond-oil in a 0.25-per-cent. solution. A similar appearance is observed in the case of a drop of mercury in a clean watch-glass, which, when coated with a thin layer of oil, retains its spherical form, notwithstanding its being rolled about on the glass. If, however, the mercury contain a small quantity of a solid metal, as, for example, lead, which forms in contact with the air a film of solid oxide, the drop will become cylindrical under the influence of a rocking motion.

With castor-oil in dilute solutions of soda, I have observed after the lapse of some hours a trace of a whitish precipitate at the lower surface of the oil-drop; but the latter is always in the shape of a sphere. When shaken, the drop becomes larger for a few minutes, probably because the agitation causes a little soap to be thrown from the interior of the drop to its surface, where it is dispersed. But so soon as this soap is dissolved in the aqueous fluid, and the common surface of the two liquids

returns to its former condition, the drop of oil will also assume its original shape.

6. *The Influence of Bile.*

For the bile that I employed in my experiments I am indebted to the kindness of my colleague W. Kühne; it was prepared by evaporating to dryness over the water-bath an alcoholic solution of crystallized ox-gall, and dissolving the residue in water.

If some solution of bile be added to the fluid in which floats a drop of olive, almond, or cod-liver oil coated with a solid soap membrane, this membrane will be dissolved; the oil-drop assumes the spherical form, and retains it after agitation. On this ability of bile to transform a solid into a fluid soap, or into a soap-solution, seems to depend its property of promoting the assimilation of fat in the animal body. Solid particles of soap on the surface of an oil-drop prevent any change of form in that surface, and thus diminish its ability to pass through animal membranes. This hindrance is removed so soon as the drop is coated with a fluid membrane. Bile has the property of promoting the spontaneous formation of an emulsion when the solid soap at the common surface of the oil and the soda-solution dissolves slowly; but it counteracts the emulsion-formation when it converts the solid soap into a liquid too rapidly. Both of those phenomena were observed by Gad.

According to the views of C. A. von Wistingshausen*, the bile, drawn through the walls of the lacteals, is accompanied by the adhering particles of fatty matter; but, as it seems to me, this theory has not up to the present been proved. The same observer claims to have found that olive-oil will rise higher in capillary tubes moistened with various liquids when those liquids contain in solution salts of the biliary acids. For moistening the tubes, water and dilute solution of potash were used, with or without the addition of albumen.

Unfortunately we are unable to gather from the descriptions of these experiments in what way the capillary glass tubes were moistened with the liquid. If the oil in rising drives before it a continuous layer or column of the liquid, my own experience is, contrary to that of von Wistingshausen, that oil will rise higher in capillary tubes wetted with water than in those moistened with solutions of bile of different degrees of concentration. In the presence of potash it may behave differently; the solid soap

* Compare J. Steiner on C. A. von Wistingshausen's "Researches on the Action of the Bile in the Absorption by Endosmose of the Neutral Fats," Du Bois-Reymond's *Archiv*, 1873, p. 139.

at the common surface of the oil and aqueous liquid renders the fluid column in the capillary tube less mobile; and therefore prevents the oil from rising so high. But by the addition of the solution of bile the soap is rendered soluble, the friction in the interior of the tube is diminished, and consequently the height to which the oil rises is indirectly increased.

It has been often stated (compare Gad, Du Bois's *Arch.* 1878, p. 202) that from experiments of this kind we obtain an idea of what takes place on a very much reduced scale with the diffusion of fluids through the capillaries of the animal membrane. But it must be remembered that the ascending action in capillary tubes depends on the nature of two surfaces—one the common surface of water or aqueous fluid and air, the other that of the same substance and oil. Now the first of these, on account of its greater surface-tension, has a much more decided influence on the capillary height than the second. And it also happens that the height varies with the curvature of the common surface of the oil and the fluid; in glass tubes the convex side of the common surface is generally turned towards the aqueous liquid. But this curvature will vary considerably in the same tube, and depends materially on the edge-angle of the common surface with the wall of the tube; and the edge-angle has very different values in the case of tubes of glass and of those of the animal membranes. Now, under normal circumstances the surfaces of the fluids of the animal body are in contact neither with glass nor with air; consequently no opinion can be formed of the operations of diffusion in the animal body by the measurement of capillary heights, which depend essentially on the common surfaces of fluids with glass and air. A closer insight into these phenomena of diffusion can only be obtained by an investigation of the physical and chemical actions at the common surface of one kind of fluid with another, or with the animal membrane itself.

7. Permanence of Emulsions. Froth.

J. Plateau (*Mém. de Brux.* xxxvii. p. 3, 1868) first showed that many liquids appear to possess greater viscosity on their outer than on their inner surface. More recently I myself (Poggendorff's *Annalen*, cxxxix. p. 71, 1870), as well as Marangoni (*Cimento* [2], v.-vi. p. 239, 1872), found that a free liquid surface in contact with the air becomes less mobile as soon as a thin film of some extraneous fluid is diffused over it. The same thing occurs when an extraneous liquid (solution of soap) is diffused over the common surface of the other liquids (oil and water).

The immobility or permanence of the common surface thus modified is explained by the fact that each opening in the film of extraneous liquid is immediately closed by the molecular forces; the surface-tension of the pure liquid in the opening is greater than that of the rest of the film contaminated by contact with the extraneous liquid. The extraneous film may also consist of a solid body, provided that it be not completely solid, but to some extent mobile. This follows from the above-described (under 5) phenomena and properties of the common surfaces of liquids and solid bodies (compare Wiedemann's *Annalen*, ii. p. 145, 1877).

In emulsions of the fixed oils in a solution of soda, the thin film of soap, each aperture in which is closed again by molecular force, prevents the oil-globules from coalescing. In emulsions of gum as prepared by the druggists, each minute oil-globule is separated from the aqueous fluid by a film of the solution of gum; for at their common surface with gum-solution the fixed oils have a less surface-tension than at that with water; this property I have proved by special measurements, as I shall have occasion to explain at greater length in another place. The longer the gum-solution remains attached to the surface of the oil, the firmer it will adhere.

When mercury is agitated with water and olive-oil, a viscous mass of a grey colour is formed: this is an emulsion of mercury, consisting of a number of very small globules of the metal, each of which is coated with a film of oil kept adhering to it by molecular force. A fissure in the oil-film of one millimetre breadth will reunite with a force which, according to my own measurements, amounts to 6.09 mgr., and is therefore comparatively great. In fact, an emulsion of this kind will endure for months. The addition of an acid breaks up the oil-film, and consequently destroys the emulsion. So-called grey ointment is an emulsion of mercury in a highly viscous fat; its permanence increases with keeping, in consequence of the mercury forming with the rancid fat a compound (mercurial soap?) which appears to diffuse itself over the common surface of the two substances.

Froth which may be seen in a solution of soap or in beer, is an emulsion of air in an aqueous fluid. Its permanence is greater in proportion as the original surface-tension of the pure liquid is decreased by an extraneous film on the free surface that is bounded by air. Homogeneous fluids containing no heterogeneous substance do not form a froth; it can be obtained from fatty but not from pure water. The force with which a rent of the breadth of a millimetre in the film of extraneous fluid on water tends to reunite, in the case of soap-

solution is 5.58 mgr., of albumen 2.40 mgr., and of a ten-per-cent. solution of tannic acid 2.88 mgr.

With organic fluid substances like albumen and solution of tannic acid, a thin membrane of the fixed substance seems to form on the surface of the air-bubbles; for they show angular protuberances, and the surface becomes less mobile; this immobile membrane promotes the durability of the emulsion. A permanent froth of this kind made with white of egg is well known in the kitchen.

With very volatile liquids, or such as are easily soluble in the original aqueous fluid, such as alcohol and ether, the froth soon subsides; for the film of extraneous liquid on the surface of the water rapidly disappears, owing either to evaporation or solution. The froth of beer is broken up by a small quantity of ether; for the tension of the common surface of that substance and air is very slight, and the surface itself tears under the dispersion of the fluid laminae forming the foam, in a similar way as in the experiments on the dispersion of soap-solution over the common surface of oil and water, described above (under 3), where particles are split off from the main mass of oil.

8. *Conclusions.*

(1) A solution of soap disperses itself over the common surface of oil and water.

(2) This dispersion causes, in the interior both of the oil and the surrounding fluid, eddies or vortex-movements, by which particles of the oil are isolated or detached, and are drawn into the surrounding fluid, where they form small globules.

(3) Very minute quantities of soap, so small that they cannot even be recognized by the microscope, are enough to produce this dispersion-phenomenon and the movements in the whole body of oil caused by it.

(4) Fixed oils containing free fatty acid form in a weak solution of soda a solid soap, which dissolves in the surrounding fluid and is dispersed over the surface of the oil.

(5) With a certain concentration of the solution of soda, and a certain solubility of the soap that is formed, the dispersion occurs at certain intervals, and detaches a large number of the small oil-globules. This explains the spontaneous formation of an emulsion observed by Joh. Gad, and the *Amœba*-like movements of the oil-globules in dilute solution of soda.

(6) The globules of oil are coated with a thin film of soap, either solid or dissolved in water; and this film, by molecular action, causes the oil-surface to be less mobile, prevents the

globules from coalescing, and materially promotes the durability of the emulsion.

(7) Apothecaries' emulsions consist of oil-globules coated with a thin film of gum, which is retained at the oil-surface by molecular force, and prevents the small globules from coalescing to form drops of oil.

(8) In the case of castor-oil an emulsion cannot be readily formed, because the soap produced by the contact of the oil with the soda-solution is highly soluble.

(9) Bile facilitates the solution of the solid soap, and on that account promotes the development of emulsion, in the fluid contents of the intestine, or under certain circumstances may impede it. But the mobility of the oil-surface is increased by the bile.

(10) From the height of the ascent in capillary tubes, or from the behaviour of fluids at their common surface with air, no conclusions can be drawn as to the phenomena that will occur at their common surface with other liquids or with solid bodies.

(11) Froth is an emulsion of air instead of one of oil. Its permanence depends on the same physical conditions as that of oil emulsions.

*XLIX. On the Photographic Method of Registering Absorption-Spectra, and its Application to Solar Physics. By Capt. W. DE W. ABNEY, R.E., F.R.S.**

THERE are certain difficulties in registering the visible absorption-spectra as observed, dependent on the eye of the observer, and on his power of representing correctly what he sees; and it is owing to these deficiencies that curious mistakes have been made in endeavouring to draw absorption-phenomena. Up to the present time it has been, comparatively speaking, useless to attempt such registration by means of photography, owing to the fact that merely one part of the spectrum was impressionable by the silver salts employed as a sensitive medium. Since my discovery that silver bromide could be prepared in such a molecular state as to be sensitive to the whole spectrum (visible, ultra violet, and ultra red†), the difficulty in the employment of photography is done away with; and it should be taken into use as much as possible, so as to eliminate the errors of eye-observations. A natural objection would arise at first sight, viz. that for the

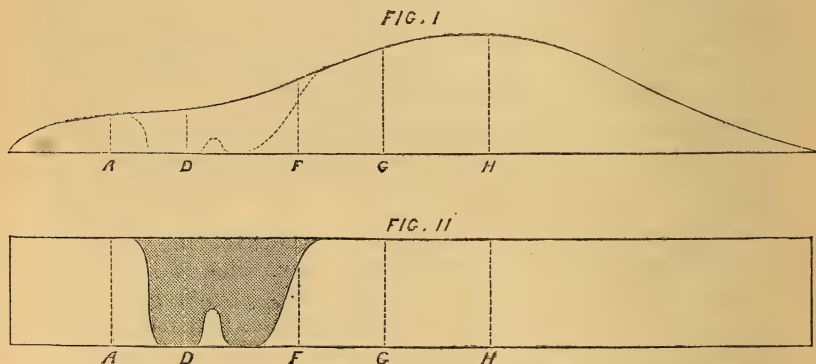
* Communicated by the Physical Society.

† Except those radiations of low amplitude and large wave-length which are emitted by bodies at ordinary temperatures.

different parts of the spectrum the sensitiveness of the silver compound is materially different, and that consequently the absorption at different parts cannot be well compared. The objection vanishes, however, at once, if ordinary precautions are taken; and as an illustration I will take a case.

The absorption of a violet (cobalt) glass was required to be registered photographically. A spectroscope having two prisms of 62° was judged to give sufficient dispersion; and a lens was used in the camera of a focal length of about 2 feet. This gave a spectrum about 4 inches long, including the visible and invisible radiations. The plate having been placed in the camera, the top half of the slit was shielded, and sunlight was reflected onto the bottom half for two minutes; the sunlight was diverted, and the absorbing medium (in this case violet glass) was placed in front of the slit*, the lower half covered up, and sunlight again reflected onto the top half of the slit for two minutes more. The plate was then developed, and a print taken from the negative. A scale of shade having been prepared, the following diagram was drawn from the measurements made with it.

The top continuous curve of fig. I. shows the intensity pro-



duced on the plate by unscreened sunlight. The dotted line in the same figure shows the curve obtained when the cobalt glass is interposed.

I would here remark that care is necessary not to introduce

* When this paper was communicated to the Physical Society, Prof. Macleod suggested that the absorption of a liquid might be better demonstrated if a wedge-shaped vessel containing it were placed in front of a longer slit, of which a small image might be produced at the focus of the collimating lens. This is quite practicable, as Professor Macleod and myself have found by actual experiment; and if the image of coloured liquid be corrected by a similar wedge of colourless liquid of nearly the same specific gravity, there is no inconvenience attaching to it.

an error, as it must be remembered that the shades produced photographically have not the same gradations as the intensity of light, as Bunsen and Roscoe first showed.

Fig. II. shows the absorption of the violet glass, on the presumption that the intensity of the radiations is equal throughout the spectrum, an assumption which is very generally made.

I have found that it is convenient in taking these spectra to modify this method. The absorption produced by potassium chromate takes somewhat of a wedge-form, shading off from darkness in the violet to total transmission at the least-refrangible end of the spectrum. If a dilute solution of this substance be interposed in each case between the source of light and the slit for half the time of exposure, we have an impression of the spectrum the varying intensity of which is less marked than if such an artifice be not employed.

I may here remark incidentally that the passage of light through an aqueous solution seems to interfere very little with the intensity of the photograph at the least-refrangible end. I had looked for a marked diminution, but have scarcely noticed it.

In photographing these absorption-spectra the source of light should be brilliant: sunlight, the image of the incandescent points of the electric light, or the oxyhydrogen light, may all be used; but I prefer sunlight, as we are enabled by the Fraunhofer lines to fix the locale of the absorption-bands more readily than with the other two.

Another application of this method is to the solar spectrum itself. Researches have shown that the bright-line spectra of incandescent compound bodies should lie in the least-refrangible end of the spectrum, and that to discover these a search must be made in these regions. As far as the visible spectrum is concerned such a search has been made; but we have yet to examine those regions which are invisible. At a low temperature it is quite possible that the compound bodies should give off vapours of the compound, whilst at high temperatures, such as that of the electric arc, they are probably dissociated. If, then, we wish to ascertain the existence of such compounds in the photosphere, we are driven to compare the solar spectrum with the bright-line spectra of the various compounds when heated at such low temperatures as those of the ordinary colourless gas- or spirit-flame. To photograph portions of such spectra (even the most "actinic" region of the spectrum) is a feat of uncommon difficulty; and it would require hours, I might say days, of exposure to impress lines in the red-region. Such an attempt would be

practically useless, as we can accomplish the same end in as many minutes by an indirect method as it would require hours by the direct method.

The following illustration will show how it can be accomplished. The top half of the slit is covered as before, and sunlight reflected onto it, and the spectrum is impressed on the photographic plate. The bottom half is next covered up, and a flame, in which the compound to be examined, is placed in front of the slit; the sunlight is then caused to traverse the flame, and a second spectrum is impressed on the plate through the top half of the slit.

New absorption-lines are thus formed in the solar spectrum, or *those already existent are intensified*, as is already well known. As an example, lithium chloride was heated in the flame, and the known line of lithium was found reversed between B and C, though absent in the spectrum of sunlight, and a faint line lying in the spectrum below the red was found intensified. By following out this plan we perhaps may eventually establish the existence of compounds in the solar photosphere. By using the light emanating from the white-hot carbon points of the magnetoelectric light to produce a continuous spectrum, and by burning the metallic compounds as before for one spectrum, and then by using sunlight to give the other spectrum, confirmatory evidence would be obtained. I may remark that I have photographed bright-line spectra of lithium, and got the same line in the ultra red as that obtained reversed. This method seems to promise to be a new weapon of attack in solar physics, more especially in this ultra-red portion.

L. On Spectra of Lightning.

By ARTHUR SCHUSTER, Ph.D., F.R.A.S.*

ALL observers of lightning-spectra agree in having seen the line-spectrum of nitrogen; but most of them have seen, in addition to this, sometimes a continuous spectrum, sometimes a band spectrum, the chemical origin of which is unknown.

The following historical summary may give an idea of our knowledge on that point.

Prof. Kundt (Pogg. Ann. cxxxv. p. 315) observed a line spectrum consisting of one or two lines in the red, some very bright ones in the green, and some less bright ones in the blue. He mentions that the lines are not always seen together.

* Communicated by the Physical Society, having been read at the Meeting on February 22nd.

Lines which in one flash appeared especially bright were not seen in another flash. The greater number of flashes, however, gave a different spectrum altogether. In the place of bright lines a great number of bands were seen; and Prof. Kundt even distinguishes two different band spectra.

Mr. John Herschel (Proc. R. S. xvii. p. 61) observed a variable continuous spectrum crossed by bright lines, which also had a variable intensity. He gives the measurements of two lines, which agree very well with nitrogen-lines.

M. Laborde (*Les Mondes*, viii. p. 219) observed some lines, especially one near E, which sometimes appeared alone. He also saw a continuous spectrum.

Dr. H. Vogel (Pogg. *Ann.* cxliii. p. 653) describes lines only; but in his list I find two which do not coincide with any bright lines in the spectrum of the electric spark taken in atmospheric air; they do, however, coincide with two bands which I have observed in some flashes of lightning, as I shall show.

Mr. J. P. Joule ('*Nature*,' vol. xvi. p. 161) also observed some spectra of lightning. Frequently there was only one bright line visible, this being coincident with the brightest nitrogen-line. At other times there were several bright lines visible, sometimes with and sometimes without the green nitrogen-line. A continuous spectrum was also observed.

Mr. H. R. Proctor ('*Nature*,' vol. xvi. pp. 161 & 220) gives some measurements of lines which do not lay claim to any accuracy. He observed also a band spectrum, which he finds not to be the band spectrum of nitrogen.

From conversation with Prof. A. Young, I learned that he also had seen a line spectrum, a band spectrum, and a continuous spectrum.

During my stay in Colorado last summer, I had some good opportunities of studying the spectra of lightning. It was my intention to get some reliable measurements of the band spectrum which I, in common with most observers, have seen; and in order to have greater chance of succeeding, I confined myself to one part of the spectrum only. The part I chose extended from $\lambda=5000$ to $\lambda=5800$, and covered, therefore, most of the yellow and green. I used a direct-vision spectroscope, with a slit movable by means of a micrometer-screw. A bright line in the principal focus of the telescope formed a fiducial mark. Under ordinary circumstances, the slit is moved until the line to be measured forms a continuation of the bright line which reaches down into the centre of the field. I found, however, that the bands I wanted to measure were nearly as broad as the thin glass bar which carries the bright line; and I used the bar therefore simply as pointer.

The measurements were always made at night; and the spectroscope was left undisturbed until the following morning, when the Fraunhofer lines in the neighbourhood were measured, so that the wave-lengths of the measurements could be interpolated.

It is of course impossible to put a pointer on a band during the instantaneous flash; but a succession of flashes allows us to put the pointer successively nearer and nearer until we see it in coincidence with the band. In this way several readings of each band were obtained. The dispersive power of the spectroscopes was such that, with a higher-power eyepiece than the one used in this investigation, the nickel-line could be seen between the two sodium-lines. The distance between the two sodium-lines was such that the two readings of the slit differed by ten divisions of the micrometer, or one tenth of a whole revolution. With sunlight I can measure easily to the tenth part of the distance between the sodium-lines. I obtained measurements on three different nights. Unfortunately, the best nights for the work occurred before the Total Solar Eclipse, which had taken me out to Colorado. A desire to save my eyes prevented me from making as good use of these nights as I otherwise should have done.

July 25th, West Las Animas.—The whole horizon seemed to be almost constantly illuminated with lightning, generally sheet-lightning. I observed about thirty or forty different flashes. I often saw the bright nitrogen-lines 5002 and 5681. I did not take any measurements of these lines; but there can hardly be a doubt as to their position. I saw in the part of the spectrum which I was observing three bands, which, however, did not always appear together. The measurements reduced to wave-lengths will be given further on. Two measurements of the bands β and γ were obtained, but one only of the band α . The greatest difference between the two measurements amounts to three times the distance between the sodium-lines. This difference must be partly accounted for by the difficulty of the observation, partly by the fact that the spectroscope had only just been unpacked after the journey; and it was found next day that it was considerably out of adjustment. The micrometer-screw, also, owing to the heat and dust, had a considerable backlash; it was taken to pieces next day and cleaned, which greatly improved it.

August 3rd, Manitou.—Clouds were coming from the west over Pike's Peak; and strong flashes of lightning, partly sheet lightning, partly forked lightning, were observed. Only two measurements were secured. One of the bands measured was β . Prof. Arthur Wright, who was present, observed that one

spot of the sky was illuminated during some flashes with a strong blue light, looking like a fluorescent light. I pointed the spectroscope to that spot, and observed a single broad band in the green. I moved the pointer on it as well as I could; but not being able to get another flash to verify the measurement, I had to take the reading. The position of this band, which I call δ , is very doubtful.

August 18th, Salt-Lake City.—I only obtained one measurement of the band γ . The kind of lightning observed differed considerably from that of the preceding nights. The lightning was nearly all forked lightning; and the bright nitrogen-line came out very strongly. The bands were but seldom seen. In one flash I saw a series of lines in the green which I had never seen before. My impression is that they were at about equal distances from each other, decreasing in strength towards the red; so that the whole made an impression similar to that of a fluted band, such as those seen in the spectrum of aluminium oxide, but shading off towards the red.

In addition to the line and band spectra, I have on many occasions seen a continuous spectrum only.

The following Table contains all the measurements I have taken. I have added in the last column numbers contained in Dr. Vogel's list of lines. It will be seen that these coincide with two of the bands I have seen.

Band.	Date.	λ	Mean.	Vogel.
α .	July 25	5592	5592	
β .	July 25	5348	5334	5341
	July 25	5329		
	Aug. 3	5325		
γ .	July 25	5175	5182	5184
	July 25	5193		
	Aug. 18	5177		
δ .	Aug. 3	5260	5260	

In trying to identify these bands with known spectra we meet with an unexpected difficulty. Two of them unfortunately admit of two different interpretations. At first sight I was struck by the close agreement of α and γ with two bands of carbonic oxide. These bands fade away towards the blue; and their sharp edges have a wave-length of 5607 and 5197. Observing with the same spectroscope, and widening the slit as I did in observing the lightning, I can produce the same impression of an unshaded band; and taking a measurement of

the centres of the bands under these circumstances, I obtain $\lambda=5579$ and $\lambda=5180$, which agree within the limits of possible errors with the above values.

The ordinary spectrum of air, however, contains a band at 5178; so that, as far as mere position is concerned, one might well be taken for the other. I was, however, under the impression that I had sometimes seen this band without the chief nitrogen double line 5002-5; and as the yellow band of carbonic oxide was also apparently present, I stated with considerable confidence when I first wrote out this paper that I had observed the spectrum of carbonic oxide. It was only when I came to work out the position of the band δ that I began to have serious doubts as to the accuracy of this conclusion. The position of the band δ , as I have said, is very doubtful; I even thought it was possible that I had taken a very bad measurement of either β or γ , and felt at first inclined to reject it altogether. On working out its wave-length, however, I found that it was coincident with one of two strong bands, which are found at the negative pole of vacuum-tubes filled with oxygen. Now the second of the two bands is nearly coincident with the yellow band of carbonic oxide; so that, of the two bands which I at first thought belonged to that gas, one might be due to nitrogen, the other to oxygen, as seen at the negative pole.

The explanation of the band β is obvious. It is the brightest of the two green lines in the low-temperature spectrum of oxygen. Its wave-length, when seen under a pressure of about a millimetre, is 5329; but under higher pressures it widens more on the less-refrangible side than towards the blue, and may well appear as a band with its centre at 5334 or even 5341, as given by Vogel.

I have not been able to obtain this band from atmospheric air in vacuum-tubes, although I have tried the experiment under various pressures. If the so-called continuous discharge is allowed to pass, the band spectrum of nitrogen alone appears; if the disruptive discharge passes, the high-temperature spectrum of oxygen is superadded to the line spectrum of nitrogen. As regards the two bands α and γ , it does not seem to me to be possible at present to decide between the two interpretations which I have given. On the one hand, it seems improbable that the slight traces of carbonic acid known to exist in the atmosphere should reveal their presence in the spectrum; but, on the other hand, it is to be remarked that oxygen vacuum-tubes, which show the band β , always reveal the slightest trace of carbonic oxide. It is exceedingly difficult, though quite possible, to obtain the band β without the bands

α and γ . The measurement of δ , however uncertain, renders it probable that the spectrum of the negative pole in oxygen forms part of the spectrum of lightning; and on the whole I should feel inclined to attribute the band α to oxygen. I have shown in my paper on the spectrum of oxygen that this spectrum of the negative pole is due to an allotropic modification of oxygen (possibly ozone), and I have been able to obtain it, though only temporarily, in the positive part of the discharge. As regards the band γ , I have some difficulty in attributing it to nitrogen, and still think it probably due to carbonic oxide. During the observations I certainly felt convinced that it did not belong to the same spectrum as the chief lines of nitrogen, and I made a note that, on the contrary, it generally appeared together with β . It seemed sometimes to be present alone, and often to form the most prominent part of the whole spectrum. As the lines of the capillary part of an oxygen-tube are also present at the negative pole, together with the bands distinctive of that pole, I can best express my observations on the band spectrum of lightning by saying that it resembles in a remarkable way the spectrum which is found at the negative pole of a vacuum-tube filled with oxygen which is slightly contaminated with carbonic oxide.

LI. *On a Property of Vulgar Fractions.*

By J. W. L. GLAISHER, M.A., F.R.S.*

§ 1. **T**HE present paper relates principally to the following property of vulgar fractions:—If all the proper fractions in their lowest terms having numerators and denominators not exceeding a given number n , be written down in order of magnitude, then each of these fractions is equal to the fraction whose numerator and denominator are respectively equal to the sum of the numerators and denominators of the fractions on each side of it; for example, if $n=7$, the fractions are

$$\frac{1}{7}, \frac{1}{6}, \frac{1}{5}, \frac{1}{4}, \frac{2}{7}, \frac{1}{3}, \frac{2}{5}, \frac{3}{7}, \frac{1}{2}, \frac{4}{7}, \frac{3}{5}, \frac{2}{3}, \frac{5}{7}, \frac{3}{4}, \frac{5}{5}, \frac{6}{7}$$

and

$$\frac{1}{6} = \frac{1+1}{7+5}, \quad \frac{1}{5} = \frac{1+1}{6+4}, \quad \frac{1}{4} = \frac{1+2}{5+7}, \text{ \&c.}$$

This property was enunciated by Mr. John Farey in the *Philosophical Magazine* for May 1816 (vol. xlvii. pp. 385–386), and was shortly afterwards proved by Cauchy. There is an-

* Communicated by the Author.

other property of the fractions arranged in order of magnitude as above, viz. that the difference of any two consecutive fractions is equal to the reciprocal of the product of their denominators; thus, for example,

$$\frac{1}{6} - \frac{1}{7} = \frac{1}{6 \cdot 7}, \quad \frac{1}{5} - \frac{1}{6} = \frac{1}{5 \cdot 6}, \quad \frac{1}{4} - \frac{1}{5} = \frac{1}{4 \cdot 5}, \text{ \&c.}$$

The first property follows at once from this; for if $\frac{a_1}{b_1}, \frac{a_2}{b_2}, \frac{a_3}{b_3}$ be any three consecutive fractions of the series, so that

$$\frac{a_2}{b_2} - \frac{a_1}{b_1} = \frac{1}{b_2 b_1}, \quad \frac{a_3}{b_3} - \frac{a_2}{b_2} = \frac{1}{b_3 b_2},$$

then

$$a_2 b_1 - a_1 b_2 = 1, \quad a_3 b_2 - a_2 b_3 = 1;$$

whence

$$a_2 b_1 - a_1 b_2 = a_3 b_2 - a_2 b_3;$$

therefore

$$a_2(b_1 + b_3) = b_2(a_1 + a_3),$$

viz.

$$\frac{a_2}{b_2} = \frac{a_1 + a_3}{b_1 + b_3},$$

which is the first property.

In the next two sections I give an elementary demonstration of these properties: §§ 4 and 5 contain an independent proof of the first property; § 6 contains an extension of the circumstances in which the properties are true; and §§ 7-13 are devoted chiefly to their history.

§ 2. *Lemma*.—If $\frac{a}{b}$ be a proper fraction in its lowest terms, and if $\frac{p}{q}, \frac{p'}{q'}$ be the nearest fractions, below and above, to $\frac{a}{b}$, having denominators less than b , then

$$p + p' = a, \quad q + q' = b;$$

and also

$$\frac{a}{b} - \frac{p}{q} = \frac{1}{bq}, \quad \frac{p'}{q'} - \frac{a}{b} = \frac{1}{q'b}.$$

For let $\frac{a}{b}$ be converted into a continued fraction, and let $\frac{p}{q}$ be the last convergent to $\frac{a}{b}$, and suppose that $\frac{p}{q} < \frac{a}{b}$, then

$$\frac{a}{b} - \frac{p}{q} = \frac{1}{bq};$$

whence

$$aq - bp = 1.$$

Now if there be a fraction $\frac{r}{s}$ ($s < b$) lying between $\frac{p}{q}$ and $\frac{a}{b}$, then

$$\frac{r}{s} - \frac{p}{q} < \frac{a}{b} - \frac{p}{q};$$

therefore

$$\frac{rq-sp}{s} < \frac{aq-bp}{b} < \frac{1}{b};$$

that is,

$$rq-sp < \frac{s}{b};$$

viz. a positive integer is less than a proper fraction, which is impossible. Therefore $\frac{p}{q}$ is the nearest fraction less than $\frac{a}{b}$ which has a denominator less than b .

Also, $\frac{a-p}{b-q}$ is the nearest fraction greater than $\frac{a}{b}$ which has a denominator less than b ; for if $\frac{r}{s}$ ($s < b$) lie between $\frac{a}{b}$ and $\frac{a-p}{b-q}$, then

$$\frac{a-p}{b-q} - \frac{r}{s} < \frac{a-p}{b-q} - \frac{a}{b};$$

viz.

$$\frac{as-ps-br+qr}{s} < \frac{aq-bp}{b} < \frac{1}{b};$$

or

$$as-ps-br+qr < \frac{s}{b},$$

which is impossible.

The proof is exactly similar if the last convergent $\frac{p}{q}$ be greater than $\frac{a}{b}$; and therefore, always, if $\frac{p}{q}$ be the last convergent to $\frac{a}{b}$, the nearest fractions to $\frac{a}{b}$, on each side, are $\frac{p}{q}$ and $\frac{a-p}{b-q}$, whence the truth of the first part of the lemma is evident.

Also since

$$\frac{a}{b} - \frac{p}{q} = \pm \frac{1}{bq}, \quad \frac{a-p}{b-q} - \frac{a}{b} = \pm \frac{1}{(b-q)b},$$

it follows that if $\frac{p}{q}, \frac{p'}{q'}$ be the nearest fractions to $\frac{a}{b}$, below and above, having denominators less than b , then

$$\frac{a}{b} - \frac{p}{q} = \frac{1}{bq}, \quad \frac{p'}{q'} - \frac{a}{b} = \frac{1}{q'b},$$

which is the second part of the lemma.

§ 3. The properties enunciated in § 1 are direct consequences of the lemma; for if the series of fractions whose numerators and denominators do not exceed n be written down in order of magnitude, and if the fractions with the denominator $n+1$ be inserted in their proper places in the series, then, if the fraction $\frac{m}{n+1}$ be inserted between $\frac{a_1}{b_1}$ and $\frac{a_2}{b_2}$, we have, by the second part of the lemma,

$$\frac{m}{n+1} - \frac{a_1}{b_1} = \frac{1}{(n+1)b_1}, \quad \frac{a_2}{b_2} - \frac{m}{n+1} = \frac{1}{b_2(n+1)};$$

for $\frac{a_1}{b_1}, \frac{a_2}{b_2}$ are the nearest fractions, below and above, to $\frac{m}{n+1}$, having less denominators than $n+1$. Also $\frac{1}{n+1}$ and $\frac{n}{n+1}$ will appear at the beginning and end of the series, and

$$\frac{1}{n} - \frac{1}{n+1} = \frac{1}{n(n+1)}, \quad \frac{n}{n+1} - \frac{n-1}{n} = \frac{1}{(n+1)n}.$$

Thus if the second property be true for all the fractions in the original series, it still remains true after the fractions with denominator $n+1$ have been inserted. It can be at once verified that the property is true for $n=2, 3, \&c.$; and therefore it is true generally. Thus the second property is proved; and the first property, which is a consequence of it, is therefore proved also. For example, if $n=6$, the series of fractions is

$$\frac{1}{6}, \frac{1}{5}, \frac{1}{4}, \frac{1}{3}, \frac{2}{5}, \frac{1}{2}, \frac{3}{5}, \frac{2}{3}, \frac{3}{4}, \frac{4}{5}, \frac{5}{6};$$

when the fractions with denominator 7 are introduced, $\frac{1}{7}$ appears at the beginning, $\frac{2}{7}$ between $\frac{1}{4}$ and $\frac{1}{3}$, $\frac{3}{7}$ between $\frac{2}{5}$ and $\frac{1}{2}$, $\frac{4}{7}$ between $\frac{1}{2}$ and $\frac{3}{5}$, $\frac{5}{7}$ between $\frac{2}{3}$ and $\frac{3}{4}$, and $\frac{6}{7}$ at the end.

Since 7 is the largest denominator, the second property is true (by the lemma) for all fractions in which this denominator is involved; so that if the property is true for $n=6$, it is true for $n=7$.

It is evident that the new fractions make their appearance at the beginning and end, and between each pair of fractions which are such that the sum of their denominators is equal to 7 ; and, generally, in the series of fractions whose numerators and denominators do not exceed n , the fractions with denominator $n+1$ make their appearance between each pair of fractions which are such that the sum of their denominators is equal to $n+1$. If, therefore, $\phi(n)$ denote the number of numbers less than n and prime to it (unity included), there will be $\phi(n+1)-2$ such pairs of fractions; and the corresponding sums of numerators will be the $\{\phi(n+1)-2\}$ numbers which are less than $n+1$ and prime to it, unity and n being excluded, as these correspond to $\frac{1}{n+1}$ and $\frac{n}{n+1}$, which appear at the beginning and end of the series.

At the beginning of the series of fractions there will be several with unit numerators; if the greatest numerator or denominator be uneven, $=2n+1$, then the series will be

$$\frac{1}{2n+1}, \frac{1}{2n}, \dots, \frac{1}{n+1}, \frac{2}{2n+1}, \frac{1}{n}, \dots$$

viz. the first fraction that has not a unit numerator will be $\frac{2}{2n+1}$; and this will appear between $\frac{1}{n+1}$ and $\frac{1}{n}$; so that there will be $n+1$ fractions with unit numerators at the beginning; and, of course, since the second half of the series of fractions is complementary to the first half, the last $n+1$ fractions will have numerators differing from their denominators by unity. If the greatest numerator or denominator be even, $=2n$, the first fraction whose numerator is not unity will be $\frac{2}{2n-1}$, and this will appear between $\frac{1}{n}$ and $\frac{1}{n-1}$; so that in this case there will be at the beginning $n+1$ fractions with unit denominators, and at the end the same number of fractions with numerators differing by unity from their denominators.

§ 4. The first property may also be proved directly, by means of the first part of the lemma, without the intervention of the property relating to the difference of two consecutive fractions, in the following manner.

It is convenient to have a name for the two fractions nearest to $\frac{a}{b}$, below and above, having denominators less than b ; and these will therefore be referred to in this section as respectively

the inferior and superior convergent of $\frac{a}{b}$, or as the convergents of $\frac{a}{b}$. If in the series of fractions the two next to $\frac{a}{b}$, on each side of it, have denominators less than b , then by the lemma they are the two convergents $\frac{p}{q}, \frac{p'}{q'}$ of $\frac{a}{b}$, and

$$p + p' = a, \quad q + q' = b.$$

In the general case, let $\frac{p}{q}, \frac{p'}{q'}$ be respectively the inferior and superior convergents of $\frac{a}{b}$, and let $\frac{u}{v}, \frac{u'}{v'}$ be the two fractions which stand next to $\frac{a}{b}$, below and above, in the series of fractions, v and v' being supposed greater than b ; so that the series of fractions is

$$\dots \frac{p}{q}, \dots \frac{u}{v}, \frac{a}{b}, \frac{u'}{v'}, \dots \frac{p'}{q'}, \dots$$

By the first part of the lemma the convergents of $\frac{u}{v}$ are $\frac{a}{b}$ and $\frac{u-a}{v-b}$; if $v-b > b$, the convergents of $\frac{u-a}{v-b}$ are $\frac{a}{b}$ and $\frac{u-2a}{v-2b}$; if $v-2b > b$, the convergents of $\frac{u-2a}{v-2b}$ are $\frac{a}{b}$ and $\frac{u-3a}{v-3b}$, &c.

Suppose that $v-(m-1)b > b$, but that $v-mb < b$ (*i. e.* suppose that $v > mb$ and $< (m+1)b$); then we must have $v-mb = q$ and $u-ma = p$; for $v-mb$ is the first denominator less than b which occurs in the descending series of fractions from $\frac{a}{b}$; and this is in fact the definition of q .

Similarly in the ascending series, we have $q' = v' - nb$, $p' = u' - na$ if $v' > nb$ and $< (n+1)b$. Also $p + p' = a$, $q + q' = b$, and therefore

$$(u-ma) + (u'-na) = a,$$

$$(v-mb) + (v'-nb) = b;$$

whence

$$u + u' = (m+n+1)a,$$

$$v + v' = (m+n+1)b;$$

so that

$$\frac{u+u'}{v+v'} = \frac{a}{b}.$$

If $\frac{u}{v} = \frac{p}{q}$ or $\frac{u'}{v'} = \frac{p'}{q'}$, we merely have the case of $m=0$ or $n=0$ respectively.

It is evident from the preceding considerations that p, q , the numerator and denominator of the inferior convergent of $\frac{a}{b}$, are the remainders when u and v are divided respectively by a and b ; and that p', q' , the numerator and denominator of the superior convergent of $\frac{a}{b}$, are the remainders when u' and v' are divided by a and b .

It was rather more convenient to define the convergents of $\frac{a}{b}$ as the fractions nearest to it, on each side, having denominators less than b : but they might equally well be defined as the fractions nearest to $\frac{a}{b}$, on each side, having numerators not exceeding a ; for, the single case of $a=1$ excepted, if $\frac{p}{q}$ be a convergent of $\frac{a}{b}$, then $p >$ or $< a$, according as $q >$ or $< b$.

§ 5. The following ten consecutive fractions, which belong to a series in which the greatest admissible numerator or denominator is 1000, will serve to illustrate the remarks in the last section:—

$$\frac{7}{220}, \frac{26}{817}, \frac{19}{597}, \frac{31}{974}, \frac{12}{377}, \frac{29}{911}, \frac{17}{534}, \frac{22}{691}, \frac{27}{848}, \frac{5}{157}.$$

Consider the fraction $\frac{12}{377}$; we have

$$\frac{31+29}{974+911} = \frac{60}{1885} = \frac{5 \cdot 12}{5 \cdot 377};$$

so that the factor 5 divides out from both numerator and denominator. To see how this is brought about by means of the lemma, we observe (i) that the convergents of $\frac{31}{974}$ are $\frac{12}{377}$ and $\frac{19}{597}$, and that the convergents of $\frac{19}{597}$ are $\frac{12}{377}$ and $\frac{7}{220}$; (ii) that the convergents of $\frac{29}{911}$ are $\frac{12}{377}$ and $\frac{17}{534}$, and that the convergents of $\frac{17}{534}$ are $\frac{12}{377}$ and $\frac{5}{157}$; (iii) that the convergents of $\frac{12}{377}$ are $\frac{7}{220}$ and $\frac{5}{157}$. Thus, considering the nu-

merators alone, the equations given by the lemma are, from (i),

$$31 = 12 + 19,$$

$$19 = 12 + 7;$$

whence

$$31 = 2 \cdot 12 + 7.$$

Similarly, from (ii),

$$29 = 12 + 17,$$

$$17 = 12 + 5;$$

whence

$$29 = 2 \cdot 12 + 5,$$

and therefore

$$31 + 29 = 4 \cdot 12 + (7 + 5) = 4 \cdot 12 + 12 \text{ from (iii), } = 5 \cdot 12.$$

And similarly, for the denominators, from (i),

$$974 = 377 + 597, \quad 597 = 377 + 220;$$

from (ii),

$$911 = 377 + 534, \quad 534 = 377 + 157;$$

from (iii),

$$220 + 157 = 377;$$

whence

$$974 + 911 = 5 \cdot 377.$$

Also, the numerators 7, 5 of the convergents of $\frac{12}{377}$ are the remainders when 31 and 29 are divided by 12; and the denominators 220, 157 are the remainders when 974 and 911 are divided by 377.

The following example, also taken from the same series, affords another illustration of the relations connecting the fractions:—

$$\frac{20}{303}, \frac{47}{712}, \frac{27}{409}, \frac{61}{924}, \frac{34}{515}, \frac{41}{621}, \frac{48}{727}, \frac{55}{833}, \frac{62}{939}, \frac{7}{106}.$$

§ 6. The reasoning of § 3 shows that the two properties are still true in the more general case in which the series consists of fractions in their lowest terms, having numerators not exceeding any given number m and denominators not exceeding any given number n , m being of course not greater than n . For consider only the second property (which includes the first), and suppose this property to be true for the series of fractions having numerators not exceeding m and denominators not exceeding $m + r$. Introduce the fractions having numerators not exceeding m and denominators equal to $m + r + 1$; then, as in § 3, the property is true for all the fractions up to

the fraction next greater than $\frac{m}{m+r+1}$ in magnitude, and the series of fractions greater in magnitude than $\frac{m}{m+r+1}$ remains unaffected. Thus if the property is true for denominators not exceeding $m+r$, it is true for denominators not exceeding $m+r+1$. But it is true for denominators not exceeding m or $m+1$, and is therefore true generally.

If, therefore, in any such series as those considered in §§ 1-5 all the fractions having numerators exceeding any given number be removed, the properties will still hold good for the series of fractions that remain. For instance, if in the example at the end of § 5 the fractions having numerators exceeding 45 be removed, there remains the series

$$\frac{20}{303}, \frac{27}{409}, \frac{34}{515}, \frac{41}{621}, \frac{7}{106},$$

for which the properties are true.

§ 7. I come now to the history of the properties stated in § 1. The first property was published by Mr. John Farey, in a letter to the *Philosophical Magazine* (vol. xlvii. 1816, pp. 385-386), entitled "On a curious Property of Vulgar Fractions." This letter commences:—"On examining lately, some very curious and elaborate Tables of 'Complete decimal Quotients,' calculated by Henry Goodwyn, Esq. of Blackheath, of which he has printed a copious specimen, for private circulation among curious and practical calculators, preparatory to the printing of the whole of these useful Tables, if sufficient encouragement, either public or individual, should appear to warrant such a step: I was fortunate while so doing to deduce from them the following general property." Mr. Farey then states the first property, viz. that if $\frac{a_1}{b_1}, \frac{a_2}{b_2}, \frac{a_3}{b_3}$ be three consecutive fractions, then $\frac{a_2}{b_2} = \frac{a_1 + a_3}{b_1 + b_3}$, and illustrates it in the case where the greatest denominator is 5. He concludes with the words, "I am not acquainted, whether this curious property of vulgar fractions has been pointed out?; or whether it may admit of any easy or general demonstration?; which are points on which I shall be glad to learn the sentiments of some of your mathematical readers."

An account of the property appeared under the title "*Propriété curieuse des fractions ordinaires*," in the *Bulletin des Sciences par la Société Philomatique de Paris* for 1816, p. 112*. An example is given in which the greatest denomi-

* By an error of paging, the page-numbers 105-112 occur twice, and p. 121 follows the second p. 112. It is the first p. 112 that is here referred to.

nator is 7. The property is stated to be taken from the *Philosophical Magazine* for May 1816; but Mr. Farey's name is not mentioned.

A proof was given by Cauchy in the same volume of the *Bulletin* (pp. 133–135), under the title “*Démonstration d'un théorème curieux sur les nombres* ;” this proof was reprinted in t. i. (1826) pp. 114–116 of his *Exercices de Mathématiques*. The first three paragraphs of the paper in the *Bulletin* are :—

“On trouve dans le dernier Numéro de ce Bulletin l'énoncé d'une propriété remarquable des fractions ordinaires observée par M. J. Farey.

“Cette propriété n'est qu'un simple corollaire d'un théorème curieux que je vais commencer par établir.

“*Théorème*.—Si, après avoir rangé dans leur ordre de grandeur les fractions irréductibles dont le dénominateur n'excède pas un nombre entier donné, on prend à volonté, dans la suite ainsi formée, deux fractions consécutives, leurs dénominateurs seront premiers entre eux, et elles auront pour différence une nouvelle fraction dont la numérateur sera l'unité.”

Cauchy thus discovers for himself and proves the second property, viz. that $\frac{a_2}{b_2} - \frac{a_1}{b_1} = \frac{1}{b_2 b_1}$, and deduces the first, or Mr. Farey's, property from it. It is clear, from the first paragraph of his paper, that he must have referred to Mr. Farey's original letter in the *Philosophical Magazine*, since, as has been mentioned, Mr. Farey's name does not occur in the account in the *Bulletin*.

§ 8. In the ‘*Educational Times*’* for 1868 Mr. C. W. Merrifield proposed the question, “Mr. Henry Goodwyn published in 1818 a table, in which all proper fractions (reduced to their lowest terms) in which the denominator did not exceed 100, nor the numerator 50, were arranged in order of magnitude. He observed the following property, a *general* proof of which is required. Let any three consecutive fractions be $\frac{N_1}{D_1}, \frac{N_2}{D_2}, \frac{N_3}{D_3}$, then $\frac{N_1 + N_3}{D_1 + D_3} = \frac{N_2}{D_2}$;” and indicated a mode of solution depending on the property $D_1 N_2 - D_2 N_1 = 1$: a solution was also given by Mr. Morgan Jenkins.

Probably all the proofs of the second property depend ultimately on the same principles. In the proof in §§ 2 and 3 an attempt has been made to render the analysis as elementary

* *Mathematical Questions with their Solutions*. From the ‘*Educational Times*,’ vol. ix. pp. 92–95.

as possible. All that is assumed is that if $\frac{p}{q}$ be the last convergent to $\frac{a}{b}$, then $aq - bp = \pm 1$. It is a well-known theorem, and one given in elementary treatises on algebra, that any convergent to a fraction is nearer to it than any other fraction having a less denominator than that of the convergent; and the proof* of this includes a proposition proved in § 2, viz. that $\frac{p}{q}$ is nearer to $\frac{a}{b}$ than any fraction on the same side of it having a less denominator than b . But it was more convenient to establish the result in § 2 independently, for the sake of completeness, as in any case it must have been shown that $\frac{a-p}{b-q}$ was nearer to $\frac{a}{b}$ than any fraction on the same side of it having a denominator less than b . The lemma in § 2, which really forms an interesting theorem, cannot of course be new, but it is certainly little known. I may mention that, as far as I am aware, the only complete investigation of the theory of the successive minima of the expression $\frac{y}{x} - \theta$, θ being a given fraction, is given by Professor H. J. S. Smith in the addition to his "Note on Continued Fractions" ('Messenger of Mathematics,' vol. vi. (May 1876) pp. 7-14).

§ 9. Mr. Henry Goodwyn, who is referred to by Mr. Farey in his letter in the Philosophical Magazine (see § 7), published in 1818 a quarto tract entitled "The first Centenary of a Series of concise and useful Tables of all the complete decimal quotients, which can arise from dividing a unit, or any whole number less than each divisor, by all integers from 1 to 1024. To which is now added a tabular series of complete decimal quotients for all the proper vulgar fractions of which, when in their lowest terms, neither the numerator, nor the denominator, is greater than 100: with the equivalent vulgar fractions prefixed. By Henry Goodwyn. London: 1818." There is an introduction (pp. v-xiv), followed by the first centenary itself, which occupies pp. 1-18. Then there is a fresh title-page for the "Tabular Series," which consists of pp. iii-vii (introduction), pp. 1-15 (the tabular series itself), and pp. 17-30 (appendix).

In 1823 Mr. Goodwyn published two octavo works, viz.:—
(1) "A tabular Series of decimal Quotients for all the proper vulgar fractions of which, when in their lowest terms, neither the numerator nor the denominator is greater than 1000.

* Todhunter's 'Algebra,' art. 610; Gross's 'Algebra,' art. 149.

London : 1823" (introduction pp. iii-v, and tabular series 1-153); (2) "A Table of the Circles arising from the division of a unit, or any other whole number, by all the integers from 1 to 1024; being all the pure decimal quotients that can arise from this source. London : 1823" (Introduction pp. iii-v, and 118 pp. of tables). Both these works were anonymous; but there is no doubt about the authorship, as the Introduction to the "Tabular series" commences, "A short specimen of the following Tabular Series of Decimal Quotients was published, by the Computer, in 1818, in 'The First Centenary of a Series of Concise and Useful Tables,'" and the tract of 1818 bears Mr. Goodwyn's name. The 'Table of Circles' was subjoined to the 'Tabular Series' of 1823, but sold also as a separate publication*.

§ 10. The 'Tabular series' of 1823 contains the first eight (and occasionally nine or even ten) digits of the decimal values of all fractions having both numerator and denominator not exceeding 1000 arranged in order of magnitude, from $\frac{1}{1000}$ to $\frac{999}{999}$ (*i. e.* of those whose decimal values begin with .0). At the conclusion is "End of Part I.;" and it was the author's intention that Part II. should contain the fractions whose decimal values begin with .1, Part III. those beginning with .2, Part IV. with .3, and Part V. with .4. Parts I-V. would thus contain the decimal values of the fractions up to $\frac{1}{2}$; and it would be unnecessary to print the other half of the table, as the arguments and results would be complementary to those in the first half. Part I. is all that was published.

The 'Table of Circles' (1823) contains all the periods or "circles" of the fractions having denominators prime to 10, from 1 up to 1024.

By means of the two tables the complete decimal value of every vulgar fraction less than $\frac{1}{10}$ having in its lowest terms a denominator not exceeding 1000 may be obtained at once. For example, from the 'Tabular Series' we find $\frac{35}{442} = .07918552$; and entering the 'Table of Circles' with 221 (*i. e.* with the residual factor of the denominator when powers of 2 and 5 have been thrown out) we can complete the period from among the "circles" of 221, by means of the "circle" which contains the digits 7918552, which is .990950226244343891402714932126696832579185520361, so that the remaining digits of the period are 036199 ... 8325.

The 'Tabular series' of 1818 is similar to that of 1823, but only includes fractions having both numerator and denominator

* Introduction to *Tabular Series* (1823), p. iv.

not exceeding 100, up to $\frac{1}{2}$; and the 'Table of Circles' which accompanies it only extends to 100, and occupies but one page. The 'First Centenary' itself * contains tables for the conversion of vulgar fractions into decimals, arranged by denominators instead of in order of magnitude as in the 'Tabular Series.'

§ 11. The arguments in the 'Tabular Series' of 1823 afford a beautiful illustration of the properties stated in § 1; and it is from this work that the examples in § 5 have been taken. It is very interesting to apply the reasoning of § 4 to groups of fractions as they stand in this Table; of course in § 5 it was convenient to select examples in which only a few fractions were involved. Mr. Goodwyn writes in the preface to the 'Tabular Series' of 1823 (p. iv):—"The Computer would draw the attention of the curious in such matters to the following remarkable property of the fractions which form the Series: viz. *In any three consecutive vulgar fractions in the table if the numerators of the extremes and the denominators respectively be added together, the sum will form the numerator and the denominator of a fraction equal to the mean.*"

On pp. iv-v of the 'Tabular Series' attached to the 'First Centenary' of 1818, Mr. Goodwyn enunciates both properties. He explains that the fractions do not form an arithmetical progression, and proceeds:—"In fact, the law of the *increase* is such, that each Fraction exceeds that which immediately precedes it by a part equal to *unity divided by the product of their two denominators*: so that the increment is anything but constant. The law, however, is invariable; and from it a ready method is derived for verifying the arrangement of the TABULAR, or any *similar, SERIES.*"

He then refers to the mode of arrangement of the fractions (which are printed zigzag fashion in two columns), and continues:—"At the same time, it serves to illustrate this *other* law in the succession of the terms of the entire Series; namely, that the *numerator* of any Fraction in it is always the same *aliquot part, or submultiple, of the sum of the numerators of the Fractions immediately preceding and immediately following it, which its denominator is of the sum of their denominators*: ... It [the 'Tabular Series'] may be *curtailed*, either by striking out *all* the Fractions that are of a given denominator, or by obliterating *all* those of which the denominators exceed a given denominator; but, while, in the *latter* case, *both* the

* It is to be observed that the title of the tract of 1818 is "The First Centenary . . . to which is now added a Tabular Series . . ." (see title in § 9); so that the 'First Centenary' of 1818 contains both the 'First Centenary' itself and also the 'Tabular Series.'

laws, which have been mentioned, will remain unaffected, in the *former* case, *neither* of them will hold throughout the remaining terms."

§ 12. In his letter quoted in § 7, Mr. Farey speaks of "some very curious and elaborate Tables of 'Complete decimal Quotients' calculated by Henry Goodwyn . . . of which he has printed a copious specimen for private circulation . . ." This appeared in the *Philosophical Magazine* for May 1816, and would seem at first sight to point to the publication of a specimen of the 'Tabular Series' previous to this date. A copy of the specimen alluded to is in the library of the Royal Society. It contains the 'First Centenary' only; and the following address is prefixed:—"The Calculator of about a Chiliad of Tables, from the application of which, in various ways, he has himself derived considerable benefit, has been induced to print the annexed Centenary as a Specimen. Encouraged likewise by Friends—not, perhaps, quite impartial,—to give them some publicity, yet still doubtful in himself whether they deserve general notice, he adopts this method, which, he trusts, will not be deemed obtrusive or impertinent, of presenting this portion of his labours to a few Individuals. To these Gentlemen, indeed, he has not, in all instances, the good fortune of being personally known, but their scientific knowledge and mathematical attainments are highly and justly appreciated; and, it is hoped, that amongst them, some will have leisure, and inclination, to honour him with their sentiments on the Specimen, which is thus submitted to their consideration; since he is anxious to confide to their decision, whether the Tables themselves are worthy of publication,—or may sink into oblivion with their Author.

"As the above is a private Address, it seems needless for him to add, that the name of anyone, who may favour him with his opinion, shall not be divulged without his express consent. HY. GOODWYN, Blackheath, Kent, March 5th, 1816."

The 'First Centenary' (pp. xiv + 18) is exactly similar to the 'First Centenary' of 1818; and as the fractions are not arranged in order of magnitude, it contains nothing that in any way suggests either of the properties that form the subject of this paper. It seems pretty clear that no part of the 'Tabular Series' was published previous to 1818; for the title-page to the tract of 1818 runs "The First Centenary . . . to which is now added a Tabular Series . . .;" and the introduction to the 'Tabular Series' (1818), commences, "Since the 'First Centenary, &c.' and its Introduction were printed, which was in March, 1816, it has appeared to the Calculator . . ."

It would thus appear that Mr. Goodwyn published no Table

for the conversion of vulgar fractions into decimals, in which the fractions were arranged in order of magnitude, prior to the 'Tabular Series' of 1818; and in this work both the properties are referred to. In the 'Tabular Series' of 1823 only the first is stated. The wording of Mr. Farey's letter implies that he had seen not only the printed specimen of 1816, but also Mr. Goodwyn's manuscript Tables. It is not clear, however, whether Mr. Farey discovered the property he enunciated without any assistance from Mr. Goodwyn; or whether, Mr. Goodwyn having remarked the property as holding good in the 'Tabular Series,' *i. e.* when the denominator is 100, Mr. Farey merely extended it to the general case of any denominator. Whoever first began to arrange the fractions in order of magnitude could scarcely fail to notice both properties; and the second, which relates to the difference of two consecutive fractions, would probably present itself first. On the whole, therefore, it seems most probable that only the extension to the general case was due to Mr. Farey. In none of Mr. Goodwyn's works is any allusion made to Mr. Farey or to Cauchy.

It seems curious that so elementary and remarkable a property of fractions should not have been discovered until 1816. It may of course be found that it had been published previously; but supposing the discovery to be due to Mr. Goodwyn and Mr. Farey, an explanation might be afforded by the fact that the 'Tabular Series' is probably the earliest Table of the kind, and that the property would not be likely to present itself to any one who had not arranged a complete series of proper fractions having denominators less than a given number in order of magnitude.

§ 13. Mr. Goodwyn's works are almost unknown; and those of 1823, which are the most important, are, as mentioned in § 9, anonymous. The only references I have seen to them are contained in Mr. Merrifield's question quoted in § 8, and in De Morgan's articles on *Tables* in the Penny and English Cyclopædias. In the latter the works of 1823 only are described, and by inadvertence the 'Tabular Series' is stated to contain all fractions "which in their lowest terms have a numerator not exceeding 99, and a denominator not exceeding 1000, in order of magnitude." In the English Cyclopædia (1861) De Morgan continues:—"Mr. Woolgar is our authority for saying that there was a previous work by Goodwyn, 'First Centenary of concise and useful Tables of Decimal Quotients' (1818) 4to. Mr. Goodwyn (of Blackheath) was an indefatigable calculator; and the preceding Tables are the only ones of the kind which are published. His manuscripts, an enormous

mass of similar calculations, came into the possession of Dr. Olinthus Gregory, and were purchased by the Royal Society at the sale of his books in 1842." Nothing is known of them, however, at the Royal Society. The Cambridge University Library contains two copies of each of Mr. Goodwyn's publications of 1818 and 1823 *, but no copy of the specimen of 1816. There is an account of Mr. Goodwyn's works in the British Association Report on Tables (Bradford, 1873, pp. 31-33), where I have erroneously attributed the property enunciated by Mr. Farey to Cauchy. A more complete description of Mr. Goodwyn's works is contained in a paper "On Circulating Decimals, with special reference to Henry Goodwyn's 'Table of Circles' and 'Tabular Series of Decimal Quotients' (London, 1818-1823)," printed in the 'Proceedings of the Cambridge Philosophical Society, vol. iii. (1879) pp. 185-206.

Trinity College, Cambridge.
February 15th, 1879.

LII. *On the proper Relative Sectional Areas for Copper and Iron Lightning-Rods.* By R. S. BROUGH†.

SO far as mere conductivity is concerned, a comparatively thin wire of either copper or iron would suffice for the loftiest conductor; but such a thin conductor would be dangerous, because it would be fused by a heavy discharge of lightning. Now the problem is to determine what relative sectional areas should be given to copper and iron rods, in order that neither should be more liable to fusion than the other.

The usual answer given is, that an iron rod should have four times the sectional area of a copper rod‡. The result is, I suppose, arrived at in the following way. The conductivity

* One of the copies of the 'First Centenary' (1818) contains the following letter, "September 16th, 1831. Mrs. Catherine Goodwyn presents to the Library of the University of Cambridge a complete set of the works of her late father, Henry Goodwyn, Esq., of Blackheath, Kent. Royal Hill, Greenwich." Mr. Goodwyn also published a few folding sheets on weights and measures &c., which are bound up at the end of this copy of the 'First Centenary.'

† Communicated by the Author, having been read before the Asiatic Society of Bengal in November 1878.

‡ War-Office Memorandum by General Sir Frederick Chapman, R.E., G.C.B.

of copper is about six times as great as that of iron; but the melting-point of iron is about 50 per cent. higher than that of copper; therefore $\frac{6.0}{1.5} = 4$ is the ratio for the sectional area of iron to copper.

This simple treatment of the problem, however, is incomplete, because it neglects to take three most important factors into consideration—namely (1) the influence of the rise of temperature in increasing the electrical resistance of the metal, (2) the difference between the “thermal capacity” or “specific heat” of copper and iron, and (3) the fact that, the iron rod being made several times more massive than the copper rod, it will require a proportionately greater quantity of heat to increase its temperature. These omissions introduce an enormous error in the result.

The effect of the passage of a discharge of lightning through the rod will be to raise its temperature.

The temperature (T) to which a given length of the rod will be raised will depend on:—

- (1) The quantity of heat developed by the discharge.
- (2) The mass of the rod.
- (3) The specific heat σ of the metal composing the rod.

This may be expressed mathematically as follows:

$$T = \text{const} \frac{H}{\sigma m};$$

where m is the mass of a unit length of the rod, which we shall assume to be uniform in sectional area throughout its length, and H is the quantity of heat developed by the discharge.

We may take $\sigma = 0.1013$ for copper, and $\sigma = 0.1218$ for iron. These figures were only verified, by Dulong and Petit, up to 300° C. It is probable, however, that their ratio, with which we are only here concerned, would not greatly alter at higher temperatures. At any rate, comparing the specific heats between 0° and 100° C. with those between 0° and 300° C., we infer that any alteration would be in favour of iron, *i. e.* that the specific heat of iron would increase in a quicker ratio than that of copper.

Adopting the centimetre as the unit of length, the mass of one centimetre of the rod $= \rho a$, where a is the sectional area of the rod in square centimetres, and $\rho = 8.9$ for copper, and $\rho = 7.8$ for iron.

Further, assuming the quantity and duration of the discharge to be constants, $H = \text{const} \times R$, where R is the resistance of the unit length of the conductor.

But $R = \frac{\lambda}{a}$, where λ is the specific resistance of the metal per cubic centimetre at its temperature of fusion.

We may take the melting-point of copper as 1400°C. , and that of wrought iron as 2000°C. *, and, in order to find λ , assume that Dr. William Siemens's formula, which he verified to 1000°C. , holds good†, viz.

$$\lambda_t = \lambda_0(0.026577 t^{\frac{1}{2}} + 0.0031443 t - 0.29751) \text{ for copper,}$$

$$\lambda_t = \lambda_0(0.072545 t^{\frac{1}{2}} + 0.0038133 t - 1.23971) \text{ for iron.}$$

The temperature t in these formulæ is to be measured from the absolute zero; so that we have $t = 1673$ for copper, and $t = 2273$ for iron.

The value of λ_0 per cubic centimetre of copper is 1.652 microhm, and per cubic centimetre of iron is 9.827 microhms‡.

Thus the value of λ_t per cubic centimetre of copper becomes about 10 microhms at 1673°C. , and per cubic centimetre of iron becomes about 107 microhms at 2273°C.

Hence

$$\left. \begin{aligned} H &= \text{const} \frac{10}{a} \text{ for copper,} \\ \text{and} \\ H &= \text{const} \frac{107}{a} \text{ for iron.} \end{aligned} \right\}$$

Therefore

$$\left. \begin{aligned} T &= \text{const} \frac{10}{0.1013 \times 8.9 \times a^2} \text{ for copper,} \\ \text{and} \\ T &= \text{const} \frac{107}{0.1218 \times 7.8 \times A^2} \text{ for iron.} \end{aligned} \right\}$$

Now, putting $T =$ temperature of fusion in each case,

$$\left. \begin{aligned} 1400 &= \text{const} \frac{11.09}{a^2} \text{ for copper,} \\ 2000 &= \text{const} \frac{112.63}{A^2} \text{ for iron.} \end{aligned} \right\}$$

Therefore

$$\begin{aligned} \left(\frac{A}{a}\right)^2 &= \frac{1400}{2000} \cdot \frac{112.63}{11.09} \\ &= 7.112, \end{aligned}$$

whence

$$\frac{A}{a} = \frac{8}{3} \text{ nearly ;}$$

* Rankine's Tables.

† Bakerian Lecture, 1871.

‡ Jenkin's Cantor Lectures, from Mathiessen's experiments.

or, the sectional area of an iron rod should be to the sectional area of a copper rod in the ratio of 8 to 3.

This result is an argument in favour of the use of iron as being the less expensive.

Calcutta, March 3, 1879.

LIII. *Experiments for determining the Correction to be added to the Length of a Cylindrical Resonant Tube to find the true Wave-length, and the Velocity of Sound in small Tubes.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I BEG to submit to your consideration for publication the accompanying memorandum of experiments made by me with the object of determining the correction to be added to the length of a cylindrical resonant tube to find the true wave-length, and the velocity of sound in small tubes.

They give results, as regards the first point, in the main confirmatory of those already arrived at by Lord Rayleigh and Mr. R. H. M. Bosanquet; but the methods of experiment followed by me being different to theirs, the notes may be of interest. As regards the second point I am not aware of any experimental results having been published.

I am, Gentlemen,

5 Compton Place,
Canonbury, N.
March 22, 1879.

Yours obediently,

D. J. BLAICKLEY.

The first method of experiment adopted by me is applicable only to tubes closed at one end, and consists in determining in a tube of indefinite length the positions of the first and second nodes of a wave excited by a fork held over its mouth, the tube having one end sunk in water, and the water-level at the position giving maximum resonance determining the position of the node. The tube used was made of thin brass, and had an internal diameter of 2.08 inches; it was slung with a pulley and counterweight over a deep vessel of water, and its height out of the water at each observation read off by a scale. The advantage of the use of water instead of a sliding solid plug is that there is not the slightest noise to interfere with the appreciation of the point of maximum resonance.

The forks used were c' 253.55 vibrations, e' 317.3, g' 380.625, b' 444.5, and e'' 507.2. Of these I had an opportunity of comparing all but the e' with Scheibler's standards in August last, through the kindness of Mr. A. J. Ellis.

The pitch of Scheibler's forks being determined at about 70° F., and the variation being about .00005 per vibration

per degree, the pitch of those I used would be as follows at 60° F. :—*c'* 253·68, *e'* 317·46, *g'* 380·81, *bb'* 444·72, and *c''* 507·45.

Table of observed lengths, in inches, from open end of tube to first node, reduced to 60° F.

No. of observation.	<i>c'</i> .	<i>e'</i> .	<i>g'</i> .	<i>bb'</i> .	<i>c''</i> .
1.	12·568	9·971	8·176	6·897	5·974
2.	12·618	9·912	8·196	6·877	5·994
3.	12·588	9·961	8·196	6·887	5·974
4.	12·608	9·921	8·216	6·897	5·994
5.	12·598	9·961	8·196	6·887	6·034
6.	12·608	9·951	8·206	6·897	6·024
7.	12·598	9·971	8·222	6·897	6·004
8.	12·608	9·981	8·192	6·893	6·024
9.	12·578	9·971	8·222	6·913	6·044
10.	12·632	9·971	8·202	6·923	6·024
11.	12·572	9·961	8·242	6·893	5·974
12.	12·632	9·951	8·192	6·910	6·024
13.	12·636	9·951	8·242	6·890	6·024
14.	12·616	9·961	8·232	6·920	5·994
15.	12·620	9·961	8·222	6·880	6·014
16.	12·630	9·961	8·212	6·910	5·974
17.	12·600	9·971	8·222	6·917	6·014
18.	12·610	9·951	8·222	6·917	6·014
19.	12·620	9·961	8·242	6·917	5·994
20.	12·620	9·981	8·222	6·917	6·014
Average...	12·608	9·959	8·214	6·902	6·006

Table of observed lengths, in inches, from open end of tube to second node, reduced to 60° F.

No. of observation.	<i>c'</i> .	<i>e'</i> .	<i>g'</i> .	<i>bb'</i> .	<i>c''</i> .
1.	39·013	31·162	25·831	21·849	19·224
2.	39·033	31·152	25·831	21·909	19·204
3.	39·023	31·142	25·871	21·899	19·194
4.	38·993	31·172	25·831	21·859	19·214
5.	39·025	31·201	25·861	21·929	19·224
6.	38·975	31·181	25·871	21·919	19·234
7.	39·025	31·162	25·920	21·959	19·224
8.	39·025	31·162	25·900	21·919	19·234
9.	39·016	31·211	25·851	21·949	19·224
10.	39·036	31·181	25·831	21·949	19·244
11.	39·026	31·169	25·866	21·947	19·206
12.	39·038	31·189	25·886	21·957	19·206
13.	39·028	31·169	25·906	21·937	19·236
14.	39·038	31·179	25·856	21·977	19·246
15.	38·988	31·199	25·896	21·927	19·216
16.	39·008	31·156	25·876	21·967	19·206
17.	39·060	31·176	25·876	21·967	19·226
18.	39·020	31·176	25·903	21·967	19·206
19.	38·990	31·186	25·883	21·977	19·246
20.	39·050	31·156	25·893	21·957	19·226
Average...	39·020	31·174	25·872	21·936	19·222

Let L = wave length;

l_1 = length from open end to first node;

l_2 = " " " second node;

x = correction for open end.

$$\text{Then } L = 2(l_2 - l_1), \text{ and } x = \frac{L}{4} - l_1 = \frac{l_2 - l_1}{2} - l_1.$$

The lengths as tabulated, however, require further slight corrections to obtain the true values of l_1 and l_2 . The disturbing influences are:—

1. The slight veiling of the open end caused by the position of the fork (equivalent in effect to a certain increase in the length of the tube).

2. The slight variations in the pitch of the forks, due to changes of temperature.

3. Capillary attraction.

4. Humidity.

I found by experiment with two organ-pipes set slightly out of unison, and with plain open cylindrical tubes sounded by blasts across their ends, that a fork held over one of two such pipes caused an easily recognized difference in the number of beats; for the forks and tube I used, the value of this correction is about .0071 inch.

For the second correction I take the variation in the pitch of the forks as being equal to .00005 per vibration per degree Fahr.

The third possible disturbing influence I found was not measurable in a tube of the diameter used.

The observations being taken on different days, the varying humidity of the air must be taken into account: I regret that I had no means of doing this at the time; but I have availed myself of the observations taken at the Royal Botanic Gardens (about a mile distant from the place where I worked) to form an estimate of the correction due to aqueous vapour.

The dates of the observations, with the values of these different corrections to reduce the results to a standard for dry air at 60° Fahr. are here given:—

Date, 1878.	Pitch.	Average tempe- rature.	Tension of vapour.	Corrections.			Sum of correc- tions. —
				1. +	2. —	4. —	
Sept. 23.	c''	61.0	.293	.0071	.0003	.0187	.0119
" 24.	bb'	61.1	.282	.0071	.0004	.0206	.0139
" 25.	g'	61.8	.260	.0071	.0007	.0221	.0157
" 26.	e'	63.0	.264	.0071	.0015	.0275	.0219
Oct. 1.	c'	63.7	.294	.0071	.0023	.0388	.0340
" 2.	c''	63.2	.308	.0071	.0031	.0612	.0572
" 7.	g'	69.3	.362	.0071	.0119	.0994	.1042
" 8.	e'	68.4	.335	.0071	.0131	.1197	.1257
" 11.	c'	62.4	.308	.0071	.0047	.1256	.1232
" 14.	bb'	62.0	.241	.0071	.0022	.0557	.0508

Rectifying the average of the tabulated observations by the value of the corrections above given, the following reduced results are obtained for dry air at 60° F.

$l_2 =$	c''	bb'	g'	e'	c'
19.1648		21.8852	25.7678	31.0483	38.8968
$l_1 =$	5.9941	6.8881	8.1983	9.9371	12.5740
$\frac{L}{2} =$	13.1707	14.9971	17.5695	21.1112	26.3228
$\frac{L}{4} =$	6.5853	7.4985	8.7848	10.5556	13.1614
	5.9941	6.8881	8.1983	9.9371	12.5740
$x =$	$.5912$	$.6104$	$.5865$	$.6185$	$.5874$
$\frac{x}{R} =$	$.568$	$.587$	$.564$	$.595$	$.565$

$$R = \frac{2.08}{2} = 1.04.$$

	Pitch.	$\frac{L}{2}$	Feet per second.	$\frac{x}{R}$
c''	$507.45 \times 2 \times 13.1707$	$=$	1113.91	$.568$
bb' ...	$444.72 \times 2 \times 14.9971$	$=$	1111.59	$.587$
g'	$380.81 \times 2 \times 17.5695$	$=$	1115.11	$.564$
e'	$317.46 \times 2 \times 21.1112$	$=$	1116.98	$.595$
c'	$253.68 \times 2 \times 26.3228$	$=$	1112.91	$.565$
	Mean of all the obser-	}	1114.10	$.576$
	vations at 60°		1083.75	
	Do. do. 32°			

The possible sources of error in the above results would appear to be chiefly these—inaccuracy in appreciation of the point of maximum resonance, and error in estimating the humidity of the air in the tube; perhaps vapour rising from the water surface would cause the air in the tube to be more nearly saturated than the surrounding air.

The differences in the values of $\frac{x}{R}$ are very slight, and appear to be due rather to errors of observation than to any variation depending upon ratio of length to diameter, at least within the limits of an octave.

The second method of experiment I adopted was to sound an open tube by means of a steady gentle blast across one end. The tube used was the same as used for the first method; and its second proper tone was sounded; so that two nodes would fall within its length: the pitch was 506.8. After the pitch was noted, a well-fitting sliding plug was inserted at the free end and moved along the tube until the two positions were found at which the note sounded agreed in pitch with that given by the open tube. The nodes thus formed were at 6.06 inches and 19.39 inches from the free open end, with temperature 66° F. and ten-

sion of vapour .389 in. Reducing to dry air at 60° , $l_1 = 6.000$; $l_2 = 19.199$; $\frac{L}{2} = 13.199$; $x = .6$; and $\frac{x}{R} = .577$.

(Pitch $= 506.8 \times 2 \times 13.199$ in. $= 1114.88$ ft. per second at 60° , or 1084.52 at 32° .)

The velocity and value of $\frac{x}{R}$ thus found agree remarkably closely with the mean of all the observations by the first method.

Regnault's experiments give a mean limiting velocity at 32° in a tube of 4.25 inches diameter, of 1071.74 feet; but probably the pipe used by him had not so smooth an interior surface as the mandril-drawn tube used in my experiments; this would doubtless cause a difference in the loss of velocity through skin-friction.

A third method of experiment I attempted to carry out, with, however, but rather uncertain results, for the purpose of determining the velocity in smooth small tubes, such as are used in brass wind instruments. It is possible to sound notes with the lips on such cylindrical tubes without any bell-mouth; I used them in this manner, both for $1\frac{1}{4}$ and $2\frac{1}{4}$ wave-lengths. Any slight inaccuracy that may possibly be due to the lips not being exactly at a nodal point is eliminated by making use of the difference in length only between the tubes of $1\frac{1}{4}$ and $2\frac{1}{4}$ wave-lengths; in this way the additional length of tube required for 1 wave-length is obtained. The tubes used were surrounded by larger tubes; and the space between the two was filled with water to prevent the heating of the tubes by the breath. The notes were held for ten seconds at a time; and between each trial air was fanned through to get rid as much as possible of the moist breath. In all, fifty observations were made with a tube of .434 in. diam., and twenty with a tube of 1.043 in. diam. The different observations with each tube did not agree so nearly as I had hoped; but the mean results show a rapid decrease in velocity with decreasing diameter of tube, as will be seen from the following summary of all the experiments.

Diameter in inches.	Velocity in feet at 60° .
.434	1092.3
1.043	1110.5
2.080	1114.1

The velocities as above given are deduced from Scheibler's standard of pitch as explained. König's standard, with which I have had an opportunity of comparing one of my forks during the last few days, would give velocities about 2 feet less.

LIV. *On the Dissipation of Energy.* By Prof. P. G. TAIT.*To Sir W. Thomson, F.R.S.*

MY DEAR THOMSON,—

I ADDRESS you as one of the Editors of the Philosophical Magazine, but also specially as the first propounder of the doctrine of the Dissipation of Energy. I do so because Prof. Clausius, in the second part of the new edition of his work on Thermodynamics, has challenged your claim to the well-known expression for the amount of heat dissipated in a non-reversible cycle. I think that the time has come for you to speak out on the subject, so as, if possible, to prevent further unnecessary discussions.

I shall endeavour, so far as I can, to keep to matters of scientific importance; but I must introduce the subject by a reference to the comments made by Prof. Clausius upon a somewhat slipshod passage (§ 178) of my little work on ‘Thermodynamics.’ That passage refers to the integral

$$\int \frac{dq}{t},$$

to which I believe Rankine first called attention, but which is essentially connected with your doctrine.

I cannot altogether complain of Prof. Clausius’s comments, because I cannot account for my having called the above integral (in the way in which I have employed it) a *positive* quantity, except by supposing that in the revision of the first proof of my book I had thoughtlessly changed the word “negative” to “positive.” This might easily happen from my having used a novel term, “practical value,” in a somewhat ambiguous manner, at one place confounding it with “realized value.” That the whole section was meant to bear the construction forced on it by Prof. Clausius is, I think, sufficiently disproved by its opening sentence, not to speak of the fact that no one in this country has so interpreted it.

But there is a graver matter involved than any such mere slips of the pen; for Prof. Clausius asserts that the method I employ (and which I certainly obtained from your paper of 1852) is inapplicable to any but reversible cycles. This, I think, is equivalent to denying altogether your claims in the matter. I therefore quote the whole passage, correcting, however, the above-mentioned slip, and slightly extending the latter part to make my meaning perfectly clear.

“§ 178. The real dynamical value of a quantity, dq , of “heat is Jdq , whatever be the temperature of the body which

“contains it. But the extreme practical value is only

$$J \frac{t-t_0}{t} dq$$

“where t is the temperature of the body, and t_0 the lowest “available temperature. This value may be written in the “form

$$Jdq - Jt_0 \frac{dq}{t}.$$

“Hence, in any cyclical process whatever, if q_1 be the whole “heat taken in, and q_0 that given out, the practical value is

$$J(q_1 - q_0) - Jt_0 \int \frac{dq}{t}.*$$

“Now the realized value is

$$J(q_1 - q_0)$$

“by the first law; and if the cycle be *reversible*, this must be “equal to the extreme practical value. Hence, in this parti- “cular case,

$$\int \frac{dq}{t} = 0.$$

“But in general this integral has a finite negative value, “because in non-reversible cycles the realized value of the heat “is always less than

$$J(q_1 - q_0) - Jt_0 \int \frac{dq}{t},$$

“which is the extreme practical value.

“Hence the amount of heat lost needlessly, *i. e.* rejected in “excess of what is necessarily rejected to the refrigerator for producing work, is

$$-t_0 \int \frac{dq}{t}.$$

“This is Thomson’s expression for the amount of heat *dissipated* “during the cycle (Phil. Mag. and Proc. R. S. E. 1852, ‘On a “Universal Tendency in Nature to Dissipation of Energy’). “It is, of course, an immediate consequence of his important “formula for the work of a perfect engine.

“[It is very desirable to have a word to express the *availa- bility* for work of the heat in a given magazine; a term for “that possession, the waste of which is called dissipation.]”

As I based the greater part of the last chapter of my work

* On this formula Prof. Clausius remarks, “Die Unrichtigkeit dieses “Resultates lässt sich leicht aus dem blossen Anblicke der Formel “erkennen”!

on your papers, mainly because they appeared to me to be greatly superior to all others on the subject in the three very important qualities of simplicity, conciseness, and freedom from hypothesis, I am anxious to know whether the above passage meets with your approval.

From Prof. Clausius's comments it appears, as I have already said, that he considers the method I have adopted from you to be one which cannot be applied except to *reversible* cycles, and which; therefore, it is absurd to employ in any argument connected with dissipation of energy.

Prof. Clausius also disputes the correctness of my reference to your paper in the *Philosophical Magazine*, as containing the above expression for the heat dissipated. You ought to be a competent authority on such a question as this.

I do not now reply to the many other remarks of Prof. Clausius, simply because they refer to myself, my motives, and my book, and not to the principles or the history of science. As the matter affects you, however, I may mention that Professor Clausius attributes to me the real authorship of the paper on "Energy" which we jointly wrote for 'Good Words,' and which has been often referred to in the *Philosophical Magazine*.

But the passage in brackets in the extract above indicates a want of proper nomenclature, which would, I think, be well met by the publication of the paper on *Thermodynamic Motivity*, read by you some years ago to the Royal Society of Edinburgh.

Yours truly,
P. G. TAIT.

38 George Square, Edinburgh,
March 17th, 1879.

Note by Sir W. Thomson on the preceding Letter.

The passage quoted, with amendments, by Professor Tait from his 'Thermodynamics,' seems to me perfectly clear and accurate. Taken in connexion with the sections which preceded it in the original, its meaning was unmistakable; and a careful reader could have found little or no difficulty in making for himself the necessary corrections with which Professor Tait now presents it. It is certainly not confined to reversible cycles; but, on the contrary, it gives an explicit expression for the amount of energy dissipated, or, as I put it, "absolutely and irrecoverably wasted" in operations of an irreversible character. My original article "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy,"

communicated to the Royal Society of Edinburgh in April 1852, and published in the 'Proceedings' of the Society for that date, and republished in the Philosophical Magazine for 1852, second half year, is a sufficient answer to the challenge referred to in the opening sentence of Professor Tait's letter.

I think Professor Tait quite right in referring also to that paper for the formula $t_0 \int \frac{dq}{t}$. The whole matter is contained

in the formula $w e^{-\frac{1}{J} \int T^{\mu} dt}$, which is given explicitly in that paper. At the top of the next page in the Philosophical Magazine reprint the following passage occurs:—"If the system of thermometry adopted be such that $\mu = \frac{J}{t + \alpha}$, that is, if we agree to call $\frac{J}{\mu} - \alpha$ the temperature of a body, for which μ is the value of Carnot's function (α and J being constants), &c.;" and on the word "adopted" the following footnote is given: "According to Mayer's 'hypothesis' this system coincides with that in which equal differences of temperature are defined as those with which the same mass of air under constant pressure has equal differences of volume, provided J be the mechanical equivalent of the thermal unit, and $\frac{1}{\alpha}$ the coefficient of expansion of air."

Here the true foundation of the absolute thermodynamic scale now universally adopted was, I believe, for the first time given. I had previously, in Part III. of my "Dynamical Theory of Heat," published in the Transactions of the Royal Society of Edinburgh, and in the Philosophical Magazine for 1852, second half-year, taking advantage of a suggestion made to me by Joule, in a letter of date December 9, 1848, shown that the assumption $\mu = \frac{J}{\alpha + t}$ reduces the formula $w e^{-\frac{1}{J} \int T^{\mu} dt}$ to $w \frac{\alpha + T}{\alpha + S}$: and I used this transformation in the concluding formulas of the article referred to by Professor Tait (corrected in the *errata* of Phil. Mag. 1853, first half-year).

It was not, however, until the experiments by Joule and myself, made in the course of the years 1852, 1853, and the early part of 1854, on the thermal effects of forcing air and other gases through porous plugs, had proved that my proposed thermodynamic scale agreed as nearly with the scale of an air-thermometer as different air-thermometers agreed with one another, that I definitively adopted it in fundamental formulas of thermodynamics. Thus, for example, in Part VI. ("Thermo-electric Currents") of my "Dynamical

Theory of Heat," published in the Transactions of the Royal Society of Edinburgh for May 1854, and in the Philosophical Magazine for 1855, first half-year, the formula

$$\frac{H_t}{t} + \frac{H_{t'}}{t'} + \dots + \frac{H_t^{(n-1)}}{t^{(n-1)}} + \frac{H_t^{(n)}}{t^{(n)}} = 0$$

is given as an equivalent for

$$\Sigma \alpha_t = \Sigma \alpha_t (1 - e^{-\frac{1}{J} \int_T^t \mu dt}),$$

which was first published in the Proceedings of the Royal Society of Edinburgh for 1851, and Phil. Mag. 1852, first half-year. Tait had actually quoted the formula from my 1854 paper in § 176 of his book, and so left absolutely no foundation for Professor Clausius' objection to his saying "This is Thomson's expression &c.," quoted in his letter above.

As to the 'Good Words' article on Energy which appeared under our joint names, Professor Tait and I are equally responsible for its contents. I claim my full share of the "scientific patriotism" commended in that article, and cannot assent to Professor Clausius' giving all the credit of it to Professor Tait.

In compliance with the concluding sentence of Professor Tait's letter, I hope in the course of a few days to write out, and send to the Philosophical Magazine for publication, a short statement of the communication on Thermodynamic Motivity which I made *vivâ voce* to the Royal Society of Edinburgh on April 3rd, 1876.

LV. On *Thermodynamic Motivity*.

By Sir W. THOMSON, F.R.S.*

AFTER having for some years felt with Professor Tait the want of a word "to express the Availability for work "of the heat in a given magazine, a term for that possession "the waste of which is called Dissipation"†, I suggested three years ago the word *Motivity* to supply this want, and made a verbal communication to the Royal Society of Edinburgh defining and illustrating the application of the word; but as the communication was not given in writing, only the title of the paper, "Thermodynamic Motivity," was published. In consequence of Professor Tait's letter to me, published in the present Number of the Philosophical Magazine, I now offer,

* Communicated by the Author.

† Tait's 'Thermodynamics,' first edition (1868), § 178: quoted also in Professor Tait's letter in the present Number of the Philosophical Magazine.

for publication in the Proceedings of the Royal Society of Edinburgh and in the Philosophical Magazine, the following short abstract of the substance of that communication.

In my paper on the Restoration of Energy from an Unequally Heated Space, published in the Philosophical Magazine in January 1853, I gave the following expression for the amount of "mechanical energy" derivable from a body, B, given with its different parts at different temperatures, by the equalization of the temperature throughout to one common temperature* T, by means of perfect thermodynamic engines,

$$W = J \iiint dx dy dz \int_T^t c dt (1 - \epsilon^{-\frac{1}{J} \int_T^t \mu dt}), \quad . \quad . \quad (1)$$

where t denotes the temperature of any point x, y, z of the body, c the thermal capacity of the body's substance at that point and that temperature, J Joule's equivalent, and μ Carnot's function of the temperature t .

Further on in the same paper a simplification is introduced thus:—

"Let the temperature of the body be measured according to an absolute scale, founded on the values of Carnot's function, and expressed by the following equation,

$$t = \frac{J}{\mu} - \alpha,$$

"where α is a constant which may have any value, but ought to have for its value the reciprocal of the expansibility of air,

* In the present article I suppose this temperature to be the given temperature of the medium in which B is placed; and thermodynamic engines to work with their recipient and rejectant organs respectively in connexion with some part of B at temperature t , and the endless surrounding matter at temperature T. In the original paper this supposition is introduced subordinately at the conclusion. The chief purpose of the paper was the solution of a more difficult problem, that of finding the value of T,—a kind of average temperature of B to fulfil the condition that the quantities of heat rejected and taken in by organs of the thermodynamic engines at temperature T are equal. The burden of the problem was the evaluation of this thermodynamic average; and I failed to remark that when the value which the solution gave for T is substituted in the formula of the text it reduces to $J \iiint dx dy dz \int_T^t c dt$, which was not instantly obvious from the analytical form of my solution, but which we immediately see must be the case by thinking of the physical meaning of the result; for the sum of the excesses of the heats taken in above those rejected by all the engines must, by the first law of thermodynamics, be equal to the work gained by the supposed process. This important simplification was first given by Professor Tait in his 'Thermodynamics' (first and second editions). It does not, however, affect the subordinate problem of the original paper, which is the main problem of this one.

“in order that the system of measuring temperature here adopted may agree approximately with that of the air-thermometer. Then we have

$$\epsilon^{-\frac{1}{J} \int_0^t \mu dt} = \frac{\alpha}{t + \alpha}, \quad \dots \dots \dots (2)$$

It was only to obtain agreement with the zero of the ordinary Centigrade scale of the air-thermometer that the α was needed; and in the joint paper by Joule and myself, published in the Transactions of the Royal Society (London) for June 1854, we agreed to drop it, and to define temperature simply as the reciprocal of Carnot's function, with a constant coefficient proper to the unit or degree of temperature adopted. Thus definitively, in equation (6) of § 5 of that paper, we took $t = \frac{J}{\mu}$, and have used this expression ever since as the expression for temperature on the arbitrarily assumed thermodynamic scale. With it we have

$$\epsilon^{-\frac{1}{J} \int_T^t \mu dt} = \frac{T}{t}; \quad \dots \dots \dots (3)$$

and by substitution (1) becomes

$$W = J \iiint dx dy dz \int_T^t c dt \left(1 - \frac{T}{t}\right). \quad \dots \dots (4)$$

Suppose now B to be surrounded by other matter all at a common temperature T. The work obtainable from the given distribution of temperature in B by means of perfect thermodynamic engines is expressed by the formula (4). If, then, there be no circumstances connected with the gravity, or elasticity, or capillary attraction, or electricity, or magnetism of B in virtue of which work can be obtained, that expressed by (4) is what I propose to call the whole Motivity of B in its actual circumstances. If, on the other hand, work is obtainable from B in virtue of some of these other causes, and if V denote its whole amount, then

$$\mathfrak{M} = V + W \quad \dots \dots \dots (5)$$

is what I call the whole Motivity of B in its actual circumstances according to this more comprehensive supposition.

We may imagine the whole Motivity of B developed in an infinite variety of ways. The one which is obvious from the formula (5) is first to keep every part of B unmoved, and to take all the work producible by perfect thermodynamic engines equalizing its temperature to T; and then keeping it rigorously at this temperature, to take all the work that can

be got from it elastically, cohesively, electrically, magnetically, and gravitationally, by letting it come to rest unstressed, dis-electrified, demagnetized, and in the lowest position to which it can descend. But instead of proceeding in this one definite way, any order of procedure whatever leading to the same final condition may be followed; and, provided nothing is done which cannot be undone (that is to say, in the technical language of thermodynamics, provided all the operations be reversible), the same whole quantity of work will be obtained in passing from the same initial condition to the same final condition, whatever may have been the order of procedure. Hence the *Motivity* is a function of the temperature, volume, figure, and proper independent variables for expressing the cohesive, the electric, and the magnetic condition of B, with the gravitational potential of B simply added (which, when the force of gravity is sensibly constant and in parallel lines, will be simply the product of the gravity of B into the height of its centre of gravity above its lowest position). So also is the *Energy* of a body B (as I first pointed out, for the case of B a fluid, in Part V. of my *Dynamical Theory of Heat*, in the *Transactions of the Royal Society of Edinburgh* for December 15, 1851, entitled, "On the Quantities of Mechanical Energy contained in a Fluid in Different States as to Temperature and Density"). Consideration of the *Energy* and the *Motivity*, as two functions of all the independent variables specifying the condition of B completely in respect to temperature, elasticity, capillary attraction, electricity, and magnetism, leads in the simplest and most direct way to demonstrations of the theorems regarding the thermodynamic properties of matter which I gave in Part III. of the *Dynamical Theory of Heat* (March 1851); in "Part VI. of *Dynamical Theory, Thermoelectric Currents*," (May 1, 1854); in a paper in the *Proceedings for 1858 of the Royal Society of London*, entitled, "On the Thermal Effect of Drawing out a Film of Liquid;" and in a communication to the *Royal Society of Edinburgh* (*Proc. R. S. E.* 1869-70), "On the Equilibrium of Vapour at the Curved Surface of a Liquid;" and in my article on the *Thermoelastic and Thermomagnetic Properties of Matter*, in the first number of the *Quarterly Journal of Mathematics* (April 1855); and in short articles in *Nichol's Cyclopædia*, under the titles "Thermomagnetism, Thermoelectricity, and Pyroelectricity," put together and republished with additions in the *Philosophical Magazine* for January 1878, under the title "On the Thermoelastic, Thermomagnetic, and Pyroelectric Properties of Matter."

It would be beyond the scope of the present article to enter

in detail into these applications, which were merely indicated in my communication to the Royal Society of Edinburgh of three years ago, as a very short and simple analytical method of setting forth the whole non-molecular theory of Thermodynamics.

University of Glasgow,
April 11, 1879.

LVI. *On the Transmission and Distribution of Energy by the Electric Current.* By C. WILLIAM SIEMENS, D.C.L., F.R.S.*

[Plate XII.]

IN the autumn of 1876, when standing below the Falls of Niagara, the first impression of wonderment at the imposing spectacle before my eyes was followed by a desire to appreciate the amount of force thus eternally spent without producing any other result than to raise the temperature of the St. Lawrence a fraction of a degree†, by the concussion of the water against the rocks upon which it falls.

The rapids below the fall present a favourable opportunity of gauging the sectional area and the velocity of the river; and from these data I calculated that the fall represents energy equivalent to nearly 17 million horse-power, to produce which by steam would require about 260 million tons of coal a year, or just about the entire amount of coal raised throughout the world.

If one fall represents such a loss of power, what must be the aggregate loss throughout the world from similar causes? and is it consistent with utilitarian principles that such stores of energy should go almost entirely to waste? But the difficulty arises, how such energy (occurring as it does for the most part in mountainous countries) is to be conducted to centres of industry and population.

Transmission by hydraulic arrangements or by compressed air would be very costly and wasteful for great distances; but it occurred to me that large amounts of energy, produced by means of the dynamo-electric current-generator, might be conveyed through a metallic conductor, such as a rod of copper fixed upon insulating supports. Such a conductor would no doubt be expensive; but, if once established, the cost of maintenance would be very small, and its power of

* Communicated by the Physical Society, having been read at the meeting on February 22.

† The vertical fall being 150 feet, the increase of temperature would be $\frac{1 \cdot 5}{7 \cdot 2} = \frac{1}{5}^{\circ}$ Fahrenheit nearly.

transmitting electric energy would be limited only by the heat generated in it through electric resistance.

In venturing to give expression to my thoughts upon this subject, in my address to the Iron and Steel Institute in March 1877, I stated that a copper rod 3 inches in diameter would be capable of transmitting energy to the extent of a thousand horse-power an hour a distance of 30 miles, there to give motion to electrodynamic engines, or to produce illumination sufficient to light up a town with 250,000 candle-power.

Although this statement was considered by many a bold one at the time it was made, I now find that a conductor such as I then described might be able to transmit three or four times the amount of power then named, and that the light producible per horse-power was also, according to our present more advanced state of knowledge, very much understated.

No serious difficulty need be apprehended as to the production of a current sufficient in amount to fill a conductor of such large proportions as here indicated. Although it would perhaps be impossible to construct a single dynamo-electric machine of sufficient power for that purpose, any number of smaller machines could be easily coupled up both for intensity and quantity to produce the desired aggregate amount.

A difficulty would, however, arise at the other end, where the electric energy was to be applied, and where it would therefore be requisite to have an arrangement for its distribution over a number of branch circuits, so that each might receive such a proportion of the total current in the main conductor as to produce the number of lights, or the amount of power intended to be supplied. An accidental increase of resistance in one or other of the branch circuits would produce the double inconvenience of starving the circuits in which such increased resistance had occurred and of supplying an excess of current to the other circuits.

In order to carry out such a system of supply, it would be necessary to have the means of so regulating the current in each branch circuit, that only a predetermined amount should be allowed to flow through the same; it would be desirable also to furnish each circuit with the means of measuring and recording the amount of electric current passed through the same in any period of time.

It is my special purpose to bring before you an instrument by which these two purposes can be accomplished. The current-regulator (as represented in Plate XII.) consists principally of a strip of metal (of mild steel or fused iron by

preference), which by its expansion and contraction regulates the current passing through it. This strip is rolled down to a thickness not exceeding 0.05 millim., and is of such a breadth that the current intended to be passed through the regulated branch circuit would raise the temperature of the strip to say 50° C.

This strip of metal (A) is stretched horizontally between a fixed support and a regulating-screw (B), at which latter the current enters, passing through the strip, and thence through a coil of German-silver wire (C) laid in the form of a collar round the centre, and connected at its other extremity with a binding-screw (D), whence the current flows on towards the lights or other apparatus to be worked by electricity. Upon its middle the strip carries a saddle of insulating material, such as ebonite, upon which rests a vertical spindle, supporting a circular metallic disk (E), with platinum contacts arranged on its upper surface. Ten or any other number of short stout wires connect the helical rheostat at equidistant points with adjustable contact-screws (F), standing above the platinum contacts on the surface of the metallic disk. These wires are supported upon the circular frame (G) of wood or other insulating material, but are free to be lifted off their support if the metallic disk should rise sufficiently to be brought into contact with the screws. These latter are so adjusted that none of them touches the metallic disk when it is in its lowest position, but that they are brought one after another into contact with the same as the disk rises; and it will be easily seen that for every additional contact-screw that is raised *seriatim* by the disk, a section of the helical rheostat between attachment and attachment is short-circuited by the metallic disk, and thus excluded from the circuit. When the disk is in its uppermost position the whole of the rheostat is short-circuited, and the regulator offers no other resistance to the current than that of the horizontal strip itself. In setting the regulator to work the regulating-screw (B) is drawn on sufficiently to bring the whole of the contact-screws into contact with the disk. The passage of the current through the strip will have the effect of raising its temperature to an extent commensurate with the electrical resistance; and in the same measure the strip itself will be elongated, and cause the spindle with the contact-disk to descend.

Another form of this instrument depends for its action upon the circumstance discovered by the Count du Moncel in 1856, and more recently taken advantage of by Mr. Edison, that the electrical resistance of carbon varies inversely with the pressure to which it is subjected. A steel wire of 0.3

millim. diameter is attached at one end to an adjusting-screw, B, and at the other to one end of a bell-crank lever, L, by means of which the pressure is brought to bear upon a pile of carbon disks, C, placed in a vertical glass tube. The current enters the instrument at the adjusting-screw B, and, passing through the wire and bell-crank lever, leaves below the pile of carbon disks. Its effect is to cause a rise of temperature in the steel wire, which, through its expansion, diminishes the pressure upon the carbon disks, and thus produces an increase in their electrical resistance. This simple apparatus thus supplies a means of regulating the strength of small currents, so as to vary only within certain narrow limits.

According to Joule's law the heat generated in the strip per unit of time depends upon its resistance, and upon the square of the current; or

$$H = C^2 R, \therefore C = \sqrt{\frac{H}{R}}.$$

On the other hand, the dissipation of heat by radiation depends upon the surface of the strip, and upon the difference between its temperature and that of the air. Therefore, in order that the current C may remain constant, it must, at every moment, be equal to the square root of the temperature divided by the resistance; and this function is performed automatically by the regulator, which throws in or takes out resistance in the manner described, according as the temperature increases or diminishes.

The regulating instrument may also be adapted to the measurement of powerful electric currents, by attaching to the end of the sensitive strip a lever, with a pencil pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork, a datum line being drawn on the strip by another pencil. The length of the ordinate between the two lines depends, in the first place, upon the current which passes at each moment, and, in the second place, upon the loss of heat by radiation from the strip.

If R' is the resistance and H' the heat with a current C' and temperature T' , then, by the law of Joule,

$$H' = R' C'^2,$$

and the loss by radiation is equal to

$$H' = (T' - T)S,$$

in which T' is the temperature of the strip, T that of the atmosphere, and S the surface of the strip.

Considering that the resistance varies as the absolute temperature of the conductor, according to a law first expressed by Helmholtz, the value of R may be put for R' for small variations of temperature; and as during an interval of constant current the heat generated and that radiated off will be equal, we obtain

$$C'^2 = (T' - T) \frac{S}{R}, \therefore C' = \sqrt{\frac{(T' - T)S}{R}} \quad \dots (1)$$

in which $T' - T$ represents the movement of the pencil, and S is constant.

For any other temperature T'' ,

$$C'' = \sqrt{\frac{(T'' - T)S}{R}}.$$

For small differences of C'' and C' ,

$$(C'' - C')^2 = 2C''(C'' - C');$$

that is to say, small variations of current will be proportional to the variations in the temperature of the strip.

To determine the value of a diagram in Weber's or other units of current, it is only necessary, if the variations are not excessive, to average the ordinates, and to determine their value by equation (1), or from a Table.

These observations may suffice to show the possibility of regulating and measuring electric currents with an ease and certainty quite equal to that obtained in dealing with currents of liquids such as gas or water; and the time may not be far distant when the use of such an instrument will also become a public necessity.

Other forms of the instrument will readily suggest themselves to the mind of the constructive engineer; but the two typical forms I have described on this occasion will suffice, I think, to show its general character.

LVII. *Products of the Oxidation of Wool. Cyano-propionic* Acid.* By J. ALFRED WANKLYN and W. J. COOPER†.

IN the course of the investigations which have led up to the moist-combustion process, we have come across some results which appear to be worth recording. On the present occasion we single out the oxidation of wool, so as to produce large quantities of a new nitrogenous acid endowed with great stability.

When wool, dissolved in water by the aid of about three

* Apparently, judging from the reaction with potash, the acid is isocyano-propionic acid.—J. A. W.

† Communicated by the Authors.

times its weight of caustic potash, is oxidized by four times its weight of permanganate of potash, there are produced carbonic acid, oxalic acid, and a certain quantity of ammonia; and when the oxidation is limited by the employment of only four times as much permanganate as wool, at least two new acids survive, one of which (*viz.* cyano-propionic acid) we have succeeded in obtaining in a state of purity.

The acids arising from oxidation under these conditions are met with in combination with potash, and mixed with the excess of caustic potash and the carbonate and oxalate of potash arising from the oxidation of the wool. The alkaline liquid is first filtered from the brown oxide of manganese resulting from the destruction of the permanganate; and then it is neutralized with sulphuric acid, and evaporated down to crystallization. After the deposit of the greater part of the sulphate of potash, the mother-liquor contains oxalate of potash, which may be precipitated by the addition of weak alcohol; and after the separation of the oxalate of potash, the alcoholic mother-liquor will be found to contain the potash-salts of at least two new organic acids. One of these new salts is very soluble in weak alcohol of 40 or 50 per cent., but almost insoluble in alcohol of 84 per cent.; and this salt, which is cyano-propionate of potash, we have investigated.

In our first attempts we endeavoured to take advantage of this character in order to purify the salt; but we afterwards found that the baryta-salt admits of a most satisfactory mode of treatment, and have accordingly resorted to the baryta-salt.

We proceed to describe our experiment:—A large Berlin porcelain dish, capacity from 5 to 6 litres, was fitted into a water-bath and heated to 100° C. One litre of water, 300 grms. of solid potash, and 100 grms. of Berlin wool were next placed in the dish, and heated and stirred until the wool had completely dissolved in the alkaline liquid. That having been done, another litre of water was added, and 400 grms. of crystals of permanganate of potash were carefully and gradually dropped into the alkaline liquid. The action was energetic: ammonia was evolved; brown hydrated binoxide of manganese was precipitated; and in a short time the colour of the permanganate had completely disappeared. The whole was then allowed to settle, and partly by decantation and partly by filtration the alkaline liquid was separated from the brown oxide of manganese.

The alkaline liquid was nearly neutralized with sulphuric acid, and evaporated until crops of sulphate of potash separated. Care was taken to wash each crop of sulphate of potash with repeated small quantities of water, so as to avoid loss of soluble

product; and the mother-liquor was mixed with a little alcohol, which threw down a quantity of oxalate of potash. Finally about 500 cub. centims. of solution, containing the new salts dissolved in alcohol of 50 to 60 per cent., was obtained. This solution was evaporated down to a syrup; and at this stage the exchange of barium for potassium was managed as follows:—To the syrup 20 grms. of H_2OSO_3 , previously diluted with about an equal volume of water, was added, so as to decompose the potash-salt. The resulting sulphate of potash was separated from the new acid by means of 500 cub. centims. of 84-per-cent alcohol, which dissolved the new acid and left behind the sulphate of potash. The alcoholic solution was mixed with 30 grms. of baryta which had been slaked, a little more baryta being added so as to render the liquid alkaline. The alcohol was evaporated off, and the resulting baryta-salt dried up in the water-bath. After this evaporation to dryness, the baryta-salt was redissolved in water, and the solution filtered and mixed with its own volume of 84-per-cent. alcohol. By this treatment a white powdery precipitate of baryta-salt was obtained. The precipitate was washed with 40-per-cent. alcohol, and afterwards pressed for some days between folds of bibulous paper.

The weight of the pressed baryta-salt, which retained alcohol and water, was 85 grms., containing 25.1 grms. of baryta-salt absolutely dry at 100°C .

One of the objects of this precipitation by means of an equal volume of 84-per-cent. alcohol (which is equivalent to the use of 40 per cent. alcohol) is the removal of a new baryta-salt which is very soluble in alcohol.

The 85 grms. of pressed baryta-salt, containing 25.1 grms. of baryta-salt, formula $(\text{C}_4\text{H}_4\text{NBaO}_2)_2, 3\text{H}_2\text{O}$, are the material from which we have prepared the acid and the set of salts about to be described.

In addition to the 25.1 grms. of baryta-salt, there were 4 grms. of baryta-salt in the alcohol employed for washing the precipitate. The yield of cyano-propionate of baryta by 100 grms. of wool was therefore 29.1 grms.; and, as will be seen on turning to the analysis, the carbon contained by the salt is 7.2 grms.

The Berlin wool employed in these experiments had been previously examined; and a combustion of it had been made in the ordinary hygrometric condition in which it was employed. 100 grms. of the wool contain 43.40 grms. of carbon.

We have likewise made quantitative determinations of the carbonic acid and the oxalic acid given by the action of four parts of permanganate of potash on one part of wool dissolved

in potash; also we have determined the oxygen consumed in the oxidation. The details of the experiments we will publish shortly; and likewise we reserve our commentary on the bearing of these results on our views as to the structure of horn and albumen.

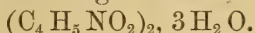
Here we give the numerical results:—

100 grms. of wool (containing 43·4 grms. of carbon), when submitted to oxidation by means of four times its weight of permanganate of potash, acting at 100° C. in alkaline solution, gave:—

Carbon in the form of CO ₂	5·9 grms.
" " C ₂ H ₂ O ₄	12·2 "
" " C ₄ H ₅ NO ₂	7·2 "
Carbon in other forms (mostly as forming acids with potash-salts very soluble in strong alcohol)	18·1 "
	43·4 grms.

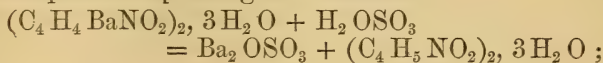
Oxygen consumed, 60·7 grms.

Cyano-propionic Acid, (C₄ H₅ NO₂)₂, 3 H₂ O.—This acid is prepared by decomposing the baryta-salt with the theoretical quantity of dilute sulphuric acid necessary to saturate the barium, separating the sulphate of baryta by filtration, and evaporating the solution of the acid to dryness in the water-bath. The solid residue, which was carefully dried in the water-bath, has the following formula—



We took 16·0 grms. of the moderately but not excessively dry baryta-salt (the purity of which had been ascertained by a complete analysis of the sample), added 75·0 cub. centims. of normal sulphuric acid (3·675 grms. of H₂ OSO₃), and obtained 8·773 grms. of washed and ignited Ba₂ OSO₃, and 9·7 grms. of the organic acid dried in a platinum dish at 100° C.

The equation expressing the reaction is



and the following is a comparison between the quantities required by the equation and those given by experiment:—

	Theory. grms.	Experiment. grms.
(C ₄ H ₄ BaNO ₂) ₂ , 3 H ₂ O	14·51	16·0
H ₂ OSO ₃ ...	3·675	3·675
Ba ₂ OSO ₃ ...	8·737	8·773
(C ₄ H ₅ NO ₂) ₂ , 3 H ₂ O	9·45	9·7

The correspondence between the sulphuric acid, sulphate of

baryta, and final organic acid will be noted. The want of accordance between the quantity of baryta-salt taken and the quantity required by the equation, as will be understood, means that the salt was not in a state of absolute dryness. The correctness of this interpretation was borne out by the observation that on ignition the acid left no appreciable fixed residue, viz. 0.774 grm. of the solid acid left, on careful ignition, 0.002 grm. of ash. The acid was also ascertained to be free from sulphuric acid.

Properties of Cyano-propionic Acid.—It is an amorphous solid, brittle at ordinary temperatures, but easily softening on being heated to 100° C. Its colour is pale brownish yellow, or straw-colour; in powder it is almost white.

It is very soluble in water and in strong alcohol. Its aqueous solution is powerfully acid, both to the taste and to litmus. It drives out carbonic acid from carbonates, and neutralizes bases completely.

The specific gravity of an aqueous solution of the acid containing 15.12 per cent. of $(C_4H_5NO_2)_2, 3H_2O$, is 1.06 at ordinary temperatures. An aqueous solution of one tenth of this strength, viz. containing 1.512 per cent., forms a lather on being shaken up, is pleasantly acid to the taste, and is capable of dissolving metallic magnesium in the cold. In presence of metallic mercury the action of the magnesium is brisk; and in the space of twenty-four hours ten cubic centimetres of the weak acid liquid confined over mercury had evolved almost the theoretical quantity of hydrogen which the magnesium was capable of displacing.

Cyano-propionic acid is attacked when it is sealed up in a tube with the acid 8-per-cent. bichromate solution and heated to 100° C. Under these conditions very little oxygen is taken up and carbonic acid is formed in considerable quantities.

In alkaline solution, when boiled with permanganate of potash, it is slowly attacked, and takes up one third of its weight of oxygen. When heated to about 200° C. with great excess of potash, it suffers a very interesting decomposition, which we are at present investigating.

When the acid is heated above 100° C. it loses weight, and becomes constant at about 140° C. After repeated heating to that temperature, the formula of the acid (judging by the loss of weight) appears to be $C_4H_5NO_2$. At temperatures above 140° C. the acid continues to lose in weight, but apparently undergoes decomposition, inasmuch as it evolves a very peculiar smell, reminding us of cyanide of ethyl. A portion of the acid which had been dried at 140° C. lost about half of its weight when heated to 220° C., and yielded a brown or black

mass insoluble in ether and water, but soluble in caustic potash. On ignition a small quantity of charcoal difficult of combustion remains. The proportion of such charcoal yielded by the acid dry at 140°C . is about one sixth of the weight of the acid. On continuing the application of heat this charcoal gradually burns away and no residue is left.

As will be seen presently, most of the salts of cyano-propionic acid are soluble in water, the only exceptions we have met with being the salts of silver, lead, and peroxide of iron. Solutions of cyano-propionates give no precipitate with salts of alumina, copper, and peroxide of mercury. Those cyano-propionates which are soluble in water are insoluble or very sparingly soluble in strong alcohol. Certain salts, as, for instance, those of lime, baryta, and magnesia, are very sparingly soluble even in 40-per-cent. alcohol.

As a rule, the salts of this acid exist in combination with water. The only salt which we have found to be nearly anhydrous is the silver salt. So far as we have ascertained, decomposition sets in before the hydrated salts give up the last portions of water.

Cyano-propionate of Baryta, $(\text{C}_4\text{H}_4\text{N BaO}_2)_2 \cdot 3\text{H}_2\text{O}$, is obtained in a state of purity, as has already been described, by precipitating its strong aqueous solution by means of an equal volume of 84-per-cent. alcohol. The precipitate is a powder almost absolutely white, which may be washed with 40-per-cent. alcohol, in which it is very sparingly soluble. It should be afterwards pressed between folds of bibulous paper and dried in the water-bath. After prolonged drying at 100°C . it has the above formula, and has furnished the following results on analysis:—

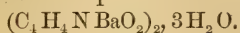
I. 0.923 grm., burnt with chromate of lead, copper-turnings being used in front of the combustion-tube, gave 0.290 grm. of water and 0.842 grm. of carbonic acid.

II. 1.471 grm. gave 0.907 grm. of sulphate of baryta.

III. 0.259 grm. gave 0.157 grm. of sulphate of baryta.

IV. 0.958 grm. gave 0.587 grm. of sulphate of baryta.

V. 0.398 grm., burnt with CuO and copper-turnings, gave 20.61 cubic centims. of nitrogen gas at 0°C . and 760 milims. N per cent. = 6.51.



			Found.				
Calculated.			I.	II.	III.	IV.	V.
C_8	96	24.81	24.88	—	—	—	—
H_{14}	14	3.62	3.49	—	—	—	—
N_2	28	7.23	—	—	—	—	6.51
Ba_2	137	35.40	—	36.26	35.64	36.03	—
O_7	112	28.94	—	—	—	—	—
		387	100.00				

At 160° C. to 170° C. the salt loses one atom of water, becoming $(C_4H_4BaNO_2)_2 \cdot 2H_2O$, but regains the water on exposure to the atmosphere at ordinary temperatures and under ordinary conditions.

It is very soluble in water, and only sparingly soluble in 40-per-cent. alcohol, the degree of solubility being about one part of the salt in one hundred parts of the alcohol of that strength.

There is a basic baryta-salt, which is obtained by adding baryta-water to the solution of the neutral salt and then precipitating by the addition of an equal volume of 84-per-cent. alcohol. The salt forms a white precipitate, which after drying at 115° C. contained 42·84 per cent. of barium. The theory for $(C_4H_4BaNO_2)_2 \cdot BaHO \cdot 3H_2O$ requires barium 43·50 per cent.

Cyano-propionate of Silver.—This salt is insoluble, or sparingly soluble in water. It was obtained by precipitating an aqueous solution of the baryta-salt by means of nitrate of silver, and formed a curdy white precipitate, which was washed, pressed between bibulous paper, and finally dried at 100° C. The dried salt was analyzed as follows:—

I. 0·397 grm., burnt with oxide of copper, copper-turnings, and a final stream of oxygen, gave 0·092 grm. of water and 0·333 grm. of carbonic acid.

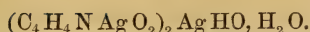
II. 1·180 grm., burnt only for water, gave 0·245 grm. of water.

III. 0·480 grm. gave, on ignition, 0·246 grm. of silver.

$(C_4H_4NAgO_2)_2 \cdot \frac{1}{2}(H_2O)$.				Found.		
Calculated.				I.	II.	III.
C ₈	96	22·80	22·88	—	—
H ₉	9	2·14	—	2·31	—
N ₂	28	6·65	—	—	—
Ag ₂	216	51·31	—	—	51·25
O _{4½}	72	17·10	—	—	—
		421	100·00			

The dry silver-salt was very hygroscopic, and absorbed 2 per cent. of water very rapidly on exposure to the air.

Apparently a basic silver-salt exists, and is obtained when basic baryta-salt is precipitated with nitrate of silver. Such a salt, dried in the water-bath, has given the following results on analysis:—



		Calculated.		Found.	
C ₈	96	17·30	17·90	—
H ₁₁	11	2·00	2·19	—
N ₂	28	—	—	—
Ag ₃	324	58·38	—	56·2
O ₆	96	—	—	—
		555			

This basic salt requires further investigation.

Cyano-propionate of Lead is insoluble, or sparingly soluble in water. It is obtained as a white precipitate on mixing an aqueous solution of the baryta-salt with an aqueous solution of acetate of lead. The precipitate was washed, pressed between bibulous paper, dried at 100° C., and analyzed.

0·463 grm., burnt with oxide of copper, gave 0·112 grm. of water and 0·374 grm. of carbonic acid.



		Calculated.		Found.
C ₈	96	22·80	22·03
H ₁₀	10	2·38	2·69
N ₂	28	—	—
Pb ₂	207	—	—
O ₅	80	—	—
		421		

Cyano-propionate of Magnesia.—This salt was prepared by precipitating the baryta-salt by an equivalent of sulphate of magnesia, separating the sulphate of baryta by filtration, and evaporating the aqueous solution of the magnesia-salt in the water-bath. It is very soluble in water, and on drying at 100° C. forms a jelly, which by very long-continued drying at 100° C. yields a brittle mass which may be powdered (giving a white powder). The powder was analyzed: 0·863 grm. yielded, on ignition, 0·132 grm. of magnesia, or 9·18 per cent. of magnesium. The formula $(C_4H_4NMgO_2)_2 \cdot 3H_2O$ requires Mg per cent. = 8·90.

The magnesia-salt is very sparingly soluble in weak alcohol, an aqueous solution yielding a precipitate when it is mixed with alcohol.

Cyano-propionate of Potash, $C_4H_4NKO_2 \cdot H_2O$ (dry at 190° C.).—We have prepared this salt by taking a weighed quantity of the acid (dry at 100° C.), dissolving it in a small quantity of water, and then exactly neutralizing the solution

with bicarbonate of potash: and we made the observation that the acid requires exactly the theoretical quantity of bicarbonate of potash in order to neutralize it.

The resulting solution of potash-salt was evaporated to dryness in the water-bath, and formed a straw-coloured transparent solid. It contained 21.68 per cent. of potassium. The formula $(C_4H_4NKO_2)_2 \cdot 5H_2O$ requires 21.47 per cent. of potassium. When the salt is deposited from strong alcohol it dries up, at $100^\circ C.$, to a solid containing rather less water, its composition being $(C_4H_4NKO_2)_2 \cdot 4H_2O$. Above the temperature of the water-bath and at temperatures below $140^\circ C.$ there is a very gradual loss of water; and by prolonged and repeated heating to $190^\circ C.$ half of the water is driven off, and the salt is found to have the formula



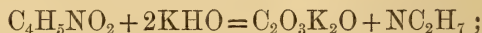
which requires K per cent. 25.21. Experiment gave K per cent. 25.8.

At temperatures higher than $190^\circ C.$ decomposition began to take place, ammonia and a smell of organic cyanides being disengaged. The potash-salt undergoes decomposition before it parts with the last atom of water of hydration.

As has already been indicated, the potash-salt is exceedingly soluble in water. It is also very soluble in 40-per-cent. alcohol; but in strong alcohol (84-per-cent. alcohol for instance) its solubility is very slight.

When the potash-salt is heated for some time to temperatures between 200° and $220^\circ C.$, with about one and a half times its weight of caustic potash, it undergoes decomposition apparently quite completely, and ethylamine is given off. On examination of the solid mass after the reaction was over we found that it contained abundance of oxalate of potash; but we found only minute traces of volatile organic acids. We proved also that the cyano-propionic acid had undergone complete decomposition. We looked carefully for succinic acid; and if present at all, it was not present in appreciable quantity.

The reaction is



and accordingly the acid is isocyano-propionic acid.

Cyano-propionate of Lime.—We prepared this salt by heating an aqueous solution of the acid with excess of finely divided carbonate of lime, filtering to remove the excess of carbonate of lime, and evaporating the solution of the lime-salt in the water-bath. The salt, after being dried at $100^\circ C.$ for some

time, forms a brittle straw-coloured mass, non-fusible at 100° C. The mass was dried at 100° C. till it became constant in weight. It was then analyzed: 0.813 grm. contained 0.1044 grm. of calcium, or Ca per cent. = 12.84. The formula $C_4H_4CaNO_2, 2H_2O$ requires Ca per cent. = 12.99.

On raising the temperature this salt shows great stability; and a short heating to 200° C. drives off hardly 4 per cent., which is less than half an atom, of water.

The salt is very soluble in water, and on mixing the aqueous solution with an equal volume of 84-per-cent. alcohol, gives an abundant powdery precipitate. No doubt this property might be taken advantage of to afford a means of purification.

LVIII. Notices respecting New Books.

Sur les Courbes dues à la Combinaison de deux Mouvements vibratoires perpendiculaires. Par M. A. TERQUEM. Lille: imprimerie L. Danel; 1879. (8vo, pp. 36.)

THIS paper is divided into four parts. In the first the Author states, by way of preliminary, the method of obtaining the equations to the Acoustic Curves by the use of Lisajous's Cylinder; in the second he investigates the properties of the curves as described on the developed surface of the cylinder; in the third he discusses the properties of the curves resulting from the projection of these cylindrical curves on a plane containing the axis—these projections being, of course, the acoustic curves themselves; and in the fourth he obtains a method of making models of the curves as drawn on the cylinder. His method is merely this:—He traces the developed curve on cardboard, and cuts it out, leaving it of a sufficient thickness; he then bends it into the required form round a wooden cylinder, and gums the edges together; when the gum is quite dry he removes the cylinder; and on duly mounting the cardboard, which retains the cylindrical shape, he has the model of the curves.

The substance of the paper is comprised in sixteen theorems, six relating to the cylindrical curves and ten to their projections. One of the chief of these theorems, relating to the projections, is the following:—"There exist in general $2mn - (m + n)$ intersections, situated on lines parallel to the axes of x and z ; some of these parallels vary when Δ varies; others remain fixed" (p. 20). In this enunciation m is the number of rotations of the cylinder, n the number of oscillations of the molecule, and $\Delta \div m$ the phase at the beginning of the time. In illustration of this theorem, the author draws the curves corresponding to ten values of Δ for $m=4$ and $n=3$, and for $m=5$ and $n=3$, *i. e.* for a fourth and a major sixth. These curves, though more complicated, correspond, of course, to those given for the octave, twelfth, and fifth in Lord Rayleigh's 'Treatise on Sound' (vol. i. p. 29), which also would serve to illus-

trate the theorem. On the whole, this is a very elegant paper on Acoustic Curves; it is needless to add that it is the work of an accomplished mathematician.

American Journal of Mathematics, Pure and Applied. Editor in chief, J. J. SYLVESTER, LL.D., F.R.S., *Corr. Mem. Inst. of France.* Associate Editor in charge, WILLIAM E. STORY, Ph.D. (Leipsic). With the Cooperation of BENJAMIN PIERCE, LL.D., F.R.S., in *Mechanics*, SIMON NEWCOMB, LL.D., F.R.S., *Corr. Mem. Inst. of France*, in *Astronomy*, and H. A. ROWLAND, C.E., in *Physics*. Vol. I. Baltimore: John Murphy and Co. (4to, pp. 388.)

The Editors of this Journal have laid down very distinctly the object they have in view in its publication, and the kind of articles which it is intended to contain. It is designed in the first instance "as a medium of communication between American mathematicians," though "its pages will always be open to contributions from abroad." Its primary object is the publication of original investigations; but in addition concise abstracts will be inserted of subjects to which special interest may attach, as well as critical and bibliographical notices and reviews of the most important recent mathematical publications.

The contents of the first Volume correspond very closely to the intention of the Editors. There will be found in it many very able original articles by American, and a considerable number by European mathematicians; a few brief historical notes and extracts; while the bibliographical notices and reviews are represented by an elaborate paper on the "Bibliography of Hyper-Space and Non-Euclidean Geometry," and an excellent review, by Prof. Pierce, of Annibale Ferrero's exposition of the method of least squares. It would take us far beyond our limits to notice even the leading articles of the volume; and there is the less need for us to do so, as some of them attracted considerable attention when the numbers containing them were published—such as Prof. Sylvester's "Application of the New Atomic Theory to the Graphical Representation of the Invariants and Covariants of Binary Quantics;" and others are sure to be studied by those who devote special attention to the subjects to which they relate—such as Mr. Hill's "Researches in the Lunar Theory," and Mr. Eddy's articles on "The Theorem of Three Moments," "The Elastic Arch," and "The Two General Methods in Graphical Statics."

The form of the work is in every way worthy of its contents: it is printed on good paper, in a clear type, and, which is a capital point, is in quarto. This last circumstance will greatly increase the expense of the publication; and as a large number of subscribers cannot, perhaps, be expected, it is gratifying to learn that a considerable part of the pecuniary risk attendant on it has been guaranteed by the Trustees of the Johns Hopkins University. We hope that the Editors will find it possible to keep the succeeding volumes up to the very high level they have reached in the first.

LIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 295.]

March 26, 1879.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "Results of a Systematic Survey (in 1878) of the Directions and Limits of Dispersion, Mode of Occurrence, and Relation to Drift-deposits of the Erratic Blocks or Boulders of the West of England and East of Wales, including a Revision of many years' previous Observations." By D. Mackintosh, Esq., F.G.S.

The author's researches lead him to the following conclusions:—Boulders from the North-Criffell range and Lake-district can be traced from the Solway Firth to near Bromsgrove (about 200 miles), and over an area in greatest breadth (from near Macclesfield to Beaumaris) of 90 miles, those from Criffell being particularly abundant near Wolverhampton. Boulders from the Arenig occupy a triangular area limited by a line drawn northward from Chirk to the Dee estuary, and to the S.E. of that town are found as far as Birmingham and Bromsgrove. The dispersion of the more distant Criffell Boulders would require submergences of from 400 to 1400 feet; of the Lake-district a little deeper; while the distant dispersion of the Arenig Boulders took place at submergences between 800 and 2000 feet. The author describes several of the more local drifts, and correlates the Lower Boulder-clay of the N.W. with the Chalky Boulder-clay of the east of England. He considers floating ice, not land-ice, to have been the agent of dispersion.

2. "On the Glaciation of the Shetland Isles." By B. N. Peach, Esq., F.G.S., and John Horne, Esq., F.G.S.

After an account of previous opinion on the subject, the authors proceeded to describe the different islands, reviewing in succession the physical features, geological structure, the direction of glaciation, and the various superficial deposits. From an examination of the numerous striated surfaces, as well as from the distribution of Boulder-clay and the dispersal of stones in that deposit, they inferred that during the period of extreme cold Shetland must have been glaciated by the Scandinavian Mer de Glace, crossing the islands from the North Sea towards the Atlantic. In the island of Unst, blocks of serpentine and gabbro are found in the Boulder-clay on the western shores derived from the rock-masses occurring on the east side of the watershed. Moreover, on the mainland between Scalloway and Fitful Head, blocks derived from the Old-Red-Sandstone formation on the eastern sea-board are abundant in the Boulder-clay on the west side of the watershed. The relative distribution of these stones in the sections on the west coast is in direct proportion to the relative areas occupied by the rocks on the east side of the watershed. It was likewise pointed out that after the

period of general glaciation Shetland nourished a series of local glaciers which radiated from the high grounds, the direction of the striæ being at variance with the older system, while the morainic deposits also differ in character from the Boulder-clay produced by the great Mer de Glace.

The authors described the order of succession in the Old Red Sandstone formation in Shetland, and referred to the discovery of an abundant series of plant-remains in rocks which have hitherto been regarded as forming part of the series of ancient crystalline rocks. The plant-remains are identical with those found in the Old-Red-Sandstone rocks in Caithness, Orkney, and Shetland, from which it was inferred that the quartzites and shales in which the fossils are imbedded must be classed with this formation. The authors also described the great series of contemporaneous and intrusive igneous rocks of Old-Red-Sandstone age, adducing evidence in proof of the great denudation which has taken place in the members of this formation in Shetland.

3. "On the Southerly Extension of the Hessle Boulder-clay in Lincolnshire." By A. J. Jukes-Browne, Esq., B.A., F.G.S.

The southern boundary of the Hessle Clay has not hitherto been satisfactorily determined. The author traces this deposit along the border of the flat fen-land in South Lincolnshire, near Burgh, Steeping, &c., and the east and west Fen. He concurs with Mr. Searles Wood in believing the clay to be the product of shore-ice along a coast-line, and that the materials were in great part derived from the older "Purple Clay." He differs, however, from that author as to the correlation of the Hessle series, thinking this more probably older than the oldest river-gravels of the S.E. of England. In an appendix a deep-well section at Boston is discussed, and reasons are given for assigning the greater part of the beds in this to the Jurassic Clays, not to the Glacial.

April 9.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On the Geological Age of the Rocks of the Southern Highlands of Ireland, generally known as 'the Dingle Beds' and 'Glen-gariff Grits.'" By Prof. E. Hull, M.A., F.R.S., F.G.S.

After reviewing the opinions of previous writers with reference to the age of these beds, including those of Hamilton, Griffith, Murchison, Kelly, Jukes, and the Officers of the Survey, which showed that great uncertainty has hitherto prevailed, the author quoted a passage of the late Prof. Jukes in which he confessedly left the determination of the age of these beds open for future examination; and he therefore determined to reinvestigate the question, bringing to bear upon it the knowledge which had since been acquired of other districts. For this purpose (and accompanied by Messrs. O'Kelly and M'Henry) he examined a series of sections, from the coast of Dingle southwards to Bantry Bay, and,

having also carefully examined the field-maps of the Survey of those districts, had arrived at the following results:—

First, that "the Dingle Beds" are perfectly conformable to, and continuous with, the Upper Silurian Beds of the Dingle promontory.

Secondly, that they are the representatives of "the Mweelrea Beds and Salrock Slates" of West Galway and Mayo, the age of which, as shown by the fossils, is Upper Silurian, and that "the Dingle Beds" may therefore be regarded as of the age of the Ludlow Rocks, but unusually developed. This view was adopted as far back as 1839 by Sir Richard Griffith.

Thirdly, that throughout the south of Ireland "the Dingle and Glengariff Beds" are disconnected from the succeeding conformable series, consisting of (c) Lower Carboniferous Slate, (b) The Upper Old Red Sandstone with *Anodonta Jukesii*, (a) The Lower Old Red Sandstones and Conglomerate, as these three conformable formations are found resting upon, and against, the Glengariff beds successively in a direction either from south to north, or from south-west to north-east, owing to a conformable overlap against the flanks of an old shelving shore formed of the Glengariff beds.

Fourthly, that at the close of the Upper-Silurian period, and after the deposition of "the Dingle and Glengariff Beds," these strata were disturbed, upraised, and denuded, and were not again submerged till the commencement of the Old Red Sandstone (a), when they were successively overlain by the beds of that formation with the succeeding ones of the Lower Carboniferous period, probably including the Carboniferous Limestone in some places.

Lastly, that it was during this period of upheaval that, as the author believes, the marine Devonian Beds (Ilfracombe and Morte series) were deposited, which accounts for their absence in the Irish area, which was either a land surface or only partially submerged. To this part of the subject the author hoped to call the attention of the Society on a future occasion.

2. "On some Three-toed Footprints from the Triassic Conglomerate of South Wales." By W. J. Sollas, Esq., M.A., F.G.S.

The author described the discovery by Mr. T. H. Thomas of some three-toed footprints in the Triassic Conglomerate at Newton Nottage, South Wales. They were stated to resemble in their most important characters the footprints of some Ratite birds, such as the Emu; and this fact, taken in connexion with the occurrence of Dinosaurian remains in the Magnesian Conglomerate of Bristol, led the author to attribute to them a Dinosaurian origin.

3. "On the Silurian District of Rhymney and Pen-y-lan, Cardiff." By W. J. Sollas, Esq., M.A., F.G.S.

The paper commences with a history of the previous observations on the district; a description of the geographical distribution, geological structure, and vertical succession of the Silurian rocks is next given. They comprise beds belonging to the Wenlock and Ludlow groups, and pass conformably upwards into the Old Red Sandstone. The district affords a good base for a measurement of the thickness of

the Old Red Sandstone on the south of the South-Wales Coalfield. This was found to be a little over 4000 feet. The thinning-out of the Old Red Sandstone and Silurian strata, together with the marked change which takes place correspondingly in the lithological characters of the latter formation on passing from the north to the south side of the coalfield were taken to indicate an approach to a shore-line. This shore-line belonged to land which, as shown by the great thickness of the Devonian beds, could not have extended far south. It corresponded to Mr. Etheridge's barrier between the Old-Red-Sandstone and Devonian seas. The sandstones with Old-Red characters, such as the Hangman Grit and the Pickwell-Down Sandstones, occurring in the Devonian formation were deposited at intervals when this barrier was submerged to a greater depth than usual. The Cornstones were stated to thin out to the south along with the other sedimentary beds of the Old Red Sandstone, and were regarded as derived from the denudation of previously upheaved limestones, such as the Bala and Hirnant. The paper concluded with a description of the characters of the more interesting rocks and fossils.

LX. *Intelligence and Miscellaneous Articles.*

ON THE OPTICAL PROPERTIES OF STARCH.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Vienna, March 31, 1879.

THE January Number of the Philosophical Magazine contains a paper by Mr. Walter Baily, in which he refers to a former paper of his, "The Optical Properties of Starch," *Phil. Mag.* for August 1876. This paper having at the time escaped my notice, I hope you will now allow me a few remarks on this subject in your Journal.

In the year 1864 I published in Poggendorff's *Annalen*, vol. cxxiii., a note, "Ueber das Kreuz, das gewisse organische Körper im polarisirten Lichte zeigen, und über die Haidinger'schen Farbenbüschel." I there showed that the cross which starch, the crystal-line lens, sections of horn, &c. give in the polarizing microscope may be explained by a radial structure of doubly refracting material.

I proved this also by experiment, by rotating a sector of mica between two Nicol prisms. Using instead of mica a dichroic mineral and only one Nicol, a cross is obtained the alternate branches of which are differently coloured. In the same paper I made use of this fact for an explanation of Haidinger's brushes.

At a later period I had a mica disk, formed of sixteen sectors, all identical and cut symmetrically to the optic axes, constructed by M. Steeg, of Homburg. This disk showed the cross very well in the old Nörrenberg apparatus, especially when by a suitable lens the bounding lines between the different sectors were rendered a little indistinct.

Another way of producing this cross depends on the fact that glass becomes doubly refracting by pressure. In pressing, therefore, a lens against a plate of glass, all the conditions for the cross are fulfilled. A little contrivance for this purpose was described by me in Carl's *Repertorium*, vol. iii. p. 376.

I am, Gentlemen,

Yours faithfully,

VIKTOR V. LANG.

ON THE MAGNETIC ROTATORY POWER OF VAPOURS.

BY E. BICHAT.

By causing the current supplied by 80 large Bunsen cells to act upon a ray of polarized light traversing vapour of sulphide of carbon, I ascertained an evident rotation of the plane of polarization. The rotation was very slight, not exceeding 15'. The first experiments which led me to the result I have just indicated were made in the course of the month of July 1878.

The apparatus was constructed by M. Ducretet, of Paris. It consists of two concentric tubes of 3.6 metres length. The inner tube is closed by parallel glass plates, and carries two tubulures furnished with cocks permitting it to be put in communication with the outside. In the annular space comprised between the two tubes a current of hot water, or a current of oil, or a current of vapour can be made to circulate. For a length of 3 metres the tube carries a series of bobbins covered with wire 3 millims. in diameter. Experience shows that under these conditions the action of the current upon the glass plates is *nil*.

By means of the same instrument I have been able to prove in the same manner an evident action of the electric current upon polarized light passing through the vapour of bichloride of tin.

I have done more: I have followed, step by step, from zero to the temperature of ebullition, the rotation of the plane of polarization produced by one and the same current acting on sulphide of carbon and bichloride of tin. I have thus ascertained that the molecular rotatory power is maintained as long as the vicinity of boiling-point of the liquid is not reached. At that moment there is a diminution much more rapid than could have been foreseen from the calculation based on the knowledge of the ratio of the densities.

I should have liked, before publishing the results of these researches to be able to establish in a rigorous fashion the relation that exists between the magnetic rotatory power of a liquid and the rotatory power of its vapour. For this it would have been necessary to be able to augment the action produced by the latter, and at the same time to improve the measuring-processes. It has not yet been possible for me to do so. I nevertheless hope soon to overcome the difficulties which have hitherto stopped me.

If I decide to publish now these still incomplete results, it is

because I have just read in a foreign scientific journal* an account of similar experiments made at Strasburg with apparatus arranged like that which I have just described. In those experiments MM. Kundt and Röntgen ascertained, without being able to measure it, the magnetic rotatory power of sulphide of carbon, sulphuretted hydrogen gas, and gaseous sulphurous acid.

There is, however, a considerable difference, in respect of the results that can be obtained by this method, between the apparatus which I use and that employed by the German physicists. This difference arises from the nature of the tube for containing the vapour. My tube is of brass, while that at Strasburg is of iron. This latter therefore constitutes a large hollow electromagnet having in its interior the gases which are to be investigated.

To show the inconvenience presented by such an arrangement, I will cite the following experiment. A tube filled with sulphide of carbon is placed between the poles of a Faraday electromagnet; it gives a rotation of $10^{\circ} 30'$. On introducing this tube inside one of the two hollow electromagnets of the same apparatus, and passing into this single electromagnet the whole current of the pile, no appreciable rotation is observed.

It is true that when the iron tube of the electromagnet is thinner the action is not entirely annulled; but it is always considerably diminished. Thus a hollow bobbin 20 centims. in length, containing a tube filled with sulphide of carbon, gives a rotation of 5° ; if an iron tube of 2.5 millims. thickness be placed in the bobbin, the rotation is not more than 1° .

These experiments, moreover, only confirm the theory of hollow magnets, given by M. Bertin nearly twenty-five years since†.—*Comptes Rendus de l'Académie des Sciences*, March 31, 1879, t. lxxxviii. pp. 712, 713.

ON AN ELECTRICAL BURNER AND BLOWPIPE.

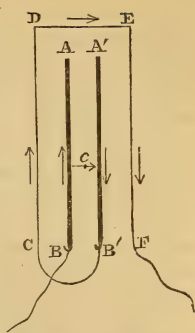
BY M. JAMIN.

The electric arc that springs between two carbon conductors is a true current. When submitted to the near influence of a current, a selenoid, or of a magnet, it undergoes an action governed by Ampère's laws, identical with that which would be undergone by any metallic conductor put in its place; but as the mass of the material of which it consists is very little, the velocities it takes are considerable. It can be attracted, repelled, displaced, fixed, caused to rotate—in a word, made to go through all the motions that can be impressed upon movable currents in electromagnetic experiments. The first action of this sort was observed by M. Quet, who projected horizontally, in the shape of a dart, a vertical arc between the two horizontal poles of an electromagnet. A multitude of similar experiments can be made; I shall today content myself with citing the following.

* Wiedemann's *Annalen*, 1879, No. 3, p. 332. See *Phil. Mag.* March 1879, p. 173.

† *Ann. de Chim. et de Phys.* (3) lviii. p. 90.

I place vertically two carbons A B, A' B', communicating with the poles of a pile or a Gramme machine; I light the arc in C by means of a small carbon introduced between the first two and then taken away. I then place behind the south pole of a magnet projected in C, or the north pole in front, or both at the same time. We know that, according to Biot and Savart's law, the element of current c must be displaced toward its right, looking at the south pole; and experiment shows that the arc immediately moves as far as the base B B' of the carbons; it reascends, on the contrary, to the summit A A' if the magnet be turned.



It then becomes fixed at the summit, but changes shape: it curves, and spreads out into a sheet with pretty intensely loud hummings. If the magnet is powerful, the arc is as it were blown upwards, and finally disappears after taking the shape of an elongated flame.

The same thing happens if we surround the two carbons with a rectangle, C D E F, traversed by the same current. Each of the parts of this rectangle cooperates in raising the arc if the direction of the current is the same in the carbons and in the rectangle, and in causing it to descend if the directions are contrary. The action is multiplied by the number of turns given to the outer wire. Four turns are sufficient to fix the arc in A A'; and it remains there, whatever be the position given to the apparatus, even when the points are directed downwards.

It is evident that in this experiment the arc can be kept in A A' and all insulating material omitted between the carbons. When the operation takes place with a continuous current of constant direction, the positive carbon burns more brightly, is more quickly consumed, and diminishes in length; the arc is maintained at and descends with its extremity. The negative carbon burns only in its interior; it diminishes in thickness, but keeps its length entire, and may serve for another time. When machines with alternating currents are employed, of which the direction changes at the same time in the carbons and in the rectangle, the action keeps the same sign; in spite of the inversions, the arc is always kept at A A'; and the carbons undergoing equal wear, their points remain always at the same level, as in the Jabloschkoff candle.

It remains to know how the arc can be lighted at the beginning, and relighted if it happens to be extinguished. For this purpose I render the carbons movable about two joints A' and B', with a spring to reunite them at their summit, and two buttresses to prevent too wide separation between them. Under these conditions the carbons repel one another, being traversed by opposite currents. Moreover C D attracts A B, and repels A' B', while E F performs the inverse action. All these effects concur to separate the carbons, which recede spontaneously. They ignite immediately the current commences, keep apart as long as it continues, to rejoin one

another whenever it ceases. In short, this is a completely automatic candle requiring only a very simple support; the lighting, the fixing at the desired distance, and the maintenance of the arc at the two points result spontaneously from the electromagnetic forces, which take upon themselves the whole of the work. It is moreover evident that these forces are proportional to the square of the intensity of the current, and can always be rendered sufficient; it is only a question of construction. M. Fernet proposed to place the carbons one in the prolongation of the other, and to take advantage of their repulsion to separate them. This repulsion was but feeble; in the solution proposed by me the action is more energetic and becomes efficient.

When the action of the rectangle is sufficient, the arc, spread out and driven beyond the points, has the appearance of a gas-flame; its length is increased. From this results a greater expenditure of electromotive force; and the amount of light is not increased proportionately; for it is known that, if the arc attains a very high temperature, it does not possess a brightness comparable with that of the carbon points. But on remarking that the arc was projected outside, I conceived the idea of receiving it upon lime, magnesia, or zircon, like the oxyhydrogen-flame in the Drummond lamp. The arc is crushed by this obstacle, keeps a constant length, and, far from consuming more electromotive force, it saves a notable portion, because it springs in a highly heated and more conductive space. On the other hand, the light, instead of disappearing skywards, where it is useless, is reflected toward the ground; this will permit the electric lamp to be placed at a great elevation, out of the usual direction of looking. Besides, the light is altogether changed: it is no longer violet, but white; it even appears greenish yellow by contrast and by the augmented intensity of the green lines of the lime; and, finally (the most valuable result of all), it is at least three times as intense as without the cap of lime. In truth, that cap must not rest upon the points; for these will fuse and penetrate the lime, and the arc will find its path inwards and shine no more. This defect can easily be remedied.

The fusion of the lime proves that the arc thus projected by a magnetic effect is capable of considerably heating all bodies; it is a real blowpipe—probably the most powerful of all. I recommend it to chemists and physicists. I shall myself have to entertain the Academy with the powerful effects that can be obtained from it.—*Comptes Rendus de l'Académie des Sciences*, March 17, 1879, t. lxxxviii. pp. 541–544.

ON THE ELECTRICAL PERFORATION OF GLASS.

BY PROF. A. VON WALTENHOFEN.

In connexion with his experiment described in 1866, and with a treatise referring to it, by E. Mach and S. Doubrava, just published, the author describes the following additional experiments:—

A thin glass plate having upon it ever so small a drop of stearine, introduced into the path of the spark of an electrical

machine, is perforated at that place, and more readily if the side on which the drop lies be turned towards the *positive* electrode.

A glass plate with bifilar suspension between the electrodes of a Holtz machine is driven by the discharge toward the negative electrode, and, indeed, with more force if the side turned to the *positive* electrode is partially coated with stearine.

Fixed points for an explanation of these facts are found by the author in the assumption, once previously enunciated, and on that occasion also advocated by G. Wiedemann, that the air molecules in the spark's path are affected, in their certainly very energetic motions, with a velocity-component directed from the positive to the negative electrode, such as was originally attributed by Plücker, and after him by Reitlinger, to the positive electricity itself.—*Kaiserliche Akademie der Wissenschaften in Wien, math.-naturw. Classe*, March 6, 1879.

ON THE PRESSURES EXERTED BY GALVANIC DEPOSITS.

BY M. BOUTY.

If we take a thermometer with a cylindrical reservoir, render it conductive by coating it with gold leaf or thinly silvering it, and employ it as the negative electrode in the decomposition of a copper-salt (for instance), the metallic deposit exerts a considerable pressure upon the reservoir; for the mercury rises in the stem the more as the deposit is thicker. And to explain this ascent one can neither invoke a local rise of temperature, which is insignificant—nor an electrical action properly so called; for the thermometrical excess has no direct relation with the intensity of the current, and persists integrally after the suppression of the latter. It depends exclusively on the more or less perfectly *metallic* quantity of the deposit, and will probably be capable of supplying the indirect measure of it. Very crystalline or coarsely granular deposits exert but a trifling compression. When we dissolve the metal with an acid the thermometer becomes normal again.

Professor Mills*, who discovered before I did, and without my being aware of it when I commenced this investigation, the fact of the contraction of thermometers, announced that copper, silver, iron, and nickel contract, and that cadmium and zinc dilate the reservoirs upon which they are applied. I have found that all metals, including zinc, always act only by pressure; but the pressure is not necessarily normal, or the same at all points, and cannot serve *directly* as a measure for the phenomenon. It is the result of a change of volume undergone by the metal in being deposited. I shall confine myself to the establishment of this point, reserving for an ulterior Note all the peculiarities I have observed.

Let us imagine that a cylinder M of external radius R and indefinite length becomes covered with a regular solid coat of which the external radius is R'. It undergoes a shrinkage, the amount of which would be a fraction a of its internal volume if the cylinder M offered no resistance; but as it does resist, a normal pressure

* Proceedings of the Royal Society, vol. xxvi. p. 504.

P is developed at all points of the contact-layer, acting from without inwards upon the cylinder, from within outwards upon the deposit. It is easy to demonstrate the formula

$$P = \frac{a}{m + \frac{1}{3} \left(\frac{8R^2}{R'^2 - R^2} + 5 \right) k}, \dots\dots\dots (1)$$

in which m represents the unit diminution of external volume of the cylinder under an external pressure equal to unity, k the compressibility-coefficient of the metal.

(1) If the deposition is produced by a current of constant intensity distributed uniformly over the whole surface of the cylinder, the weight of copper deposited upon unit length has for its expression (D designating the density of the copper, and p a constant)

$$pt = \pi(R'^2 - R^2)D, \dots\dots\dots (2)$$

whence

$$P = \frac{\frac{a}{m + \frac{5}{3}k} t}{t + \frac{8}{3} \frac{k}{m + \frac{5}{3}k} \frac{\pi D}{p} R^2} = \frac{At}{t + B} \dots\dots\dots (3)$$

I have verified that not only my experiments, but also those of Prof. Mills, are very exactly represented by empiric expressions of this form.

(2) The limit A towards which the pressure for a deposit of indefinite thickness tends is independent of R ; but B is not so: the shorter the radius R , the more rapidly does the pressure approach its limit. Experiment shows, in fact, that the contraction of an almost linear thermometer is very rapid; while I have observed only a trifling contraction of a large alcohol thermometer of 3 centims. diameter, although it had an extremely capillary stem.

A thermometer, the cross section of which is a very flat ellipse, will be submitted to pressures rapidly increasing at the extremities of the major axis of the ellipse, where the curvature is considerable; and its section will become more nearly circular; the mercury will descend in the stem, while it would rise if the same thermometer had been compressed in a piezometer.

(3) I have had made, by M. Alvergnyat, some cylindrical thermometers with much-elongated reservoirs, of known internal and external radii, and swollen at the origin of the stem, so that they could be fitted, in place of the gas-reservoir, into the apparatus constructed by M. Ducretet for the experiments of M. Cailletet. After determining experimentally their internal, from which I determined by calculation their external compressibility m , I submitted them to coppering in the centre of a Daniell's cell of the same height as the reservoir. Then observing from hour to hour their excess, I was able to determine empirically the coefficients A and B of the formula (3), and hence to deduce k . The mean of fifteen series of experiments, made with three different thermometers, gave:—

Thermometer 1.	$k=0\cdot0000012179$
„ 2.	$0\cdot0000012245$
„ 3.	$0\cdot0000012360$
Mean ..	$0\cdot0000012351$

M. Regnault found directly for cold-beaten red copper
 $k=0\cdot000001317$.

(4) The diminution of volume varied between much wider limits than k . The highest value I have calculated is $a=0\cdot000865$. Admitting this number, the greatest pressure that could be developed by a deposit of copper on an absolutely resisting cylinder ($m=0$) would be nearly 300 atmospheres. In reality I have not yet observed pressures above 100 or 110 atmospheres.

I am still pursuing these researches, for which M. Jamin has placed at my disposal all the resources of his laboratory.—*Comptes Rendus de l'Académie des Sciences*, March 13, 1879, t. lxxxviii. pp. 714-716.

NEW ESTIMATE OF SUN'S DISTANCE.

BY PLINY EARLE CHASE, LL.D.

In accordance with the principles of my spectral harmonics, Lockyer's fundamental "basic line" (4215 ten-millionths of a millimetre) gives the following equation:—

$$\frac{\text{Earth's orbital unit}}{\text{fundamental unit}} = \left(\frac{\odot \text{ radius}}{\oplus \text{ radius}} \right)^3.$$

This gives 93,700,000 miles for Sun's distance.

The "orbital unit" is the mean orbital movement of Earth while a body at Earth's equatorial surface falls through the increment of the fundamental line.

Lockyer's "basic lines," Peirce's meteoric hypothesis, and my demonstration of the influence of light in world-building and molecular grouping lead to the equation of mass—

$$\text{Jupiter}^3 = \text{Sun} \times \text{Earth} \times \text{Saturn}.$$

This equation gives the following values:—

$$\begin{aligned} \text{Sun's mass} &= 328,600, \\ \text{„ distance} &= 92,549,000 \text{ miles,} \\ \text{„ parallax} &= 8''\cdot8322. \end{aligned}$$

Haverford College, Pennsylvania,
 March 25, 1879.

PLINY E. CHASE.

CONTRIBUTION TO THE THEORY OF THE MICROPHONE.

BY HERMANN ARON.

The microphone is based, as is well known, upon the fact that vibrations produce in it alterations of the resistance, whence arise fluctuations of current which are perceived by means of a telephone.

In the following the case shall be treated of the fluctuations of resistance being indefinitely small in comparison with the total resistance; and the reaction of the vibrating plate upon the conduction, by which at all events only current-waves of inconsiderable strength compared with the original ones are produced, shall be neglected. The equation representing what takes place then becomes

$$[JW = E - Q \frac{dJ}{dt}.$$

Here E signifies the electromotive force of the battery employed, W the resistance, J the intensity of the current, and Q the electrodynamic potential of the circuit upon itself.

We now put $W = W_0 + w$, $J = J_0 + i$. W_0 and J_0 denote resistance and current-intensity in the state of repose, w and i their variations during the vibrations; w and i are, according to our hypothesis, small quantities. After this substitution our equation becomes

$$(J_0 + i)(W_0 + w) = E - Q \frac{d(J_0 + i)}{dt}.$$

If we take into account that $J_0 W_0 = E$, that iw is a small quantity of the second order, which in comparison with those of the first order we will neglect, and that $\frac{d(J_0 + i)}{dt} = \frac{di}{dt}$, our equation assumes the form

$$J_0 w + W_0 i + Q \frac{di}{dt} = 0. \dots\dots\dots (1)$$

To this equation we can attach the following remark. If w_1 and i_1 , as well as w_2 and i_2 , are two systems of waves which satisfy the equation, then $w_1 + w_2$ and $i_1 + i_2$ also satisfy it; that is, the different wave-systems are superposed without mutual disturbance. The microphone must necessarily possess this property if most of the influences operating upon it are not to express themselves entirely or for the most part as noises; and not only so, but it follows from the assumption of very small changes of resistance in proportion to the total resistance, because thereby the equation became linear; consequently we see that the fulfilment of this condition is also necessary in practice for the good rendering of a sound.

If we now analyze each vibration into summands according to Fourier's series, we need only treat the summands singly. Accordingly let $w = A \sin \frac{t}{T} 2\pi$, let the current-wave belonging to it be

$$i = B \sin \left(\frac{t}{T} 2\pi + \delta \right),$$

so that we assume the phase-change δ ; this, introduced into equation (1), yields

$$J_0 A \sin \frac{t}{T} 2\pi + W_0 B \sin \left(\frac{t}{T} 2\pi + \delta \right) + \frac{2\pi Q B}{T} \cos \left(\frac{t}{T} 2\pi + \delta \right) = 0.$$

If we develop the left side according to $\sin \frac{t}{T}$ and $\cos \frac{t}{T}$ and put their factors singly $=0$, we get two equations for B and δ :—

$$J_0 A + W_0 B \cos \delta - \frac{2\pi Q B}{T} \sin \delta = 0;$$

$$W_0 B \sin \delta + \frac{2\pi Q B}{T} \cos \delta = 0.$$

From the last equation it follows that

$$\tan \delta = - \frac{2\pi Q}{W_0 T} \dots\dots\dots (2)$$

The change of phase is therefore the greater the smaller T is—that is, the higher the tone is. For the amplitude we find

$$B = \frac{A J_0 \cos \delta}{W_0}$$

or

$$B = \frac{A E}{W_0^2} \cdot \frac{1}{\sqrt{1 + \frac{4\pi^2 Q^2}{W_0^2 T^2}}}$$

From this formula it follows that the amplitudes are not all altered in equal measure, but they become proportionally smaller for tones for which $\frac{4\pi^2 Q^2}{W_0^2 T^2}$ is greater. But this becomes smaller for T greater; that is, the higher the tone the smaller will be its amplitude; and this is so much the more the case the greater $\frac{4\pi^2 Q^2}{W_0^2}$ is—that is, the less the resistance and the greater the value of Q the electrodynamic potential. Large coils, therefore, especially with iron cores, operate prejudicially.

In his article "Telephon und Klangfarbe"*, Helmholtz showed that with the telephone also it depends on the expression $\frac{4\pi^2 Q^2}{W_0^2 T^2}$, but in exactly the inverse sense, so that in the telephone the higher tones have the advantage over the deeper ones—thus giving the peculiar result, *that in the telephone the tone is heightened, but in the microphone it is made deeper.*

A combination of the telephone with the microphone can now be produced in which the electric conveyance operates generally no change in the tone. This combination is to serve for the giving, while for the receiving instrument a telephone is made use of in the usual manner. If we further denote by M the fluctuation of the magnetic moment in the telephone of the giver, we find the following as the equation for the current-waves excited by means of such a system, in accordance with the above way of considering the subject:—

$$J_0 w_0 + W_0 i = - \frac{dM}{dt} - Q \frac{di}{dt}.$$

* Wied. Ann. v. p. 448 (1878).

And putting

$$w = A \sin \frac{t}{A} 2\pi,$$

$$M = B \sin \frac{t}{T} 2\pi,$$

$$i = C \sin \left(\frac{t}{T} 2\pi + \delta \right),$$

we get for the determination of C and δ the following equations,

$$\frac{2\pi}{T} QC \sin \delta - W_0 C \cos \delta = J_0 A,$$

$$W_0 C \sin \delta + \frac{2\pi}{T} QC \cos \delta = -\frac{2\pi}{T} B,$$

from which follows

$$C \sin \delta = \frac{\frac{2\pi}{T} (J_0 A Q - W_0 B)}{W_0^2 + \frac{4\pi^2}{T^2} Q^2},$$

$$C \cos \delta = -\frac{J_0 W_0 A + \frac{4\pi^2}{T^2} B Q}{W_0^2 + \frac{4\pi^2}{T^2} Q^2}.$$

If the ratios be chosen (as can be done in various ways) so that

$$J_0 A Q - W_0 B = 0,$$

then

$$\delta = 0$$

and

$$C = -J_0 A.$$

Thus the electrical conveyance will not introduce *any alteration either of the phase or in height or depth of tone.*—Wiedemann's *Annalen*, 1879, No. 3, vi. pp. 403-407.

Charlottenburg, December 1878.

ON THE VARIATION OF THE THERMAL CONDUCTIVITY OF METALS WITH TEMPERATURE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Dr. Hopkinson has kindly pointed out to me, in my last paper published in your recent issues (March and April), a slip of a fundamental character, which I am sorry to say renders the latter half of that paper questionable, if not, as I fear, erroneous. I hope shortly to be able to find time to go through the calculations again and make the necessary modifications; but in the meantime I must beg your readers to treat the paper as if it had not yet appeared.

I am, Gentlemen,

Your obedient Servant,

April 7, 1879.

OLIVER J. LODGE.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JUNE 1879.

LXI. *On the Thermal Conditions and on the Stratification of the Antarctic Ice.* By O. FISHER, *Clk., M.A., F.G.S.**

SIR WYVILLE THOMSON delivered a lecture at Glasgow, November 23, 1876, "On the Condition of the Antarctic"†. In it he gave a most interesting and graphic account of observations made during the visit of the 'Challenger' to high southern latitudes. Towards the conclusion of the lecture, the author advanced some speculations about the probable thickness and physical conditions of the antarctic ice-cap. There appeared also in the 'Quarterly Journal of Science' for January last an article from the pen of Dr. Croll, which contained certain criticisms upon the conclusions arrived at by Sir Wyville Thomson, and giving reasons why the ice-cap may be considerably thicker than that philosopher supposes.

I venture to think that there are a few points left in a somewhat uncertain state by both these gentlemen, which a little mathematical treatment, of by no means an abstruse nature, may assist in clearing up.

The first of these relates to the limit of thickness which the effects of temperature might impose on the antarctic ice.

Dr. Croll remarks that there are but three sources from which the ice-cap can receive an appreciable amount of heat, viz. (1) the air above, (2) the earth beneath, (3) the work of compression and friction. The last of these he dismisses as inconsiderable, and without doubt rightly so.

* Communicated by the Author.

† 'Nature,' vol. xv. pp. 102, 120.

Sir Wyville Thomson remarks that "it is not easy to see why the temperature of the earth's crust, under a widely extended and practically permanent ice-sheet of great thickness, should ever fall below the freezing-point." Our first inquiry therefore shall be :—

(1) *If a level sheet of ice rest upon the earth, its upper surface being maintained at a uniform mean temperature lower than the freezing-point, to find the thickness beyond which melting at the lower surface must take place.*

It is assumed that the form of the ice-cap is permanent; so that we may consider that the masses, whether of rock or ice, have arrived at that state in which the temperature is also permanent. Probably any horizontal movement in the ice, so long as the thickness at any place remains unaltered, will only very slightly affect this supposition—because, if there be sliding of the parts one over another, ice abstracted by horizontal movement will be replaced by ice at the same temperature.

We know that there is a continual flow of heat out of the earth. This flow is such that it is found to raise the temperature of the strata of the earth by about $\frac{1}{60}$ of a degree Fahr. per foot on descending. This average increase prevails equally wherever observations have been taken. Even at Yakoutzk, where the ground is perpetually frozen beyond the depth pierced, the same rate obtains. This may therefore be admitted as an empirical fact. But the following reasoning may perhaps be accepted as explanatory of this uniformity of rate.

If the temperature of the surface of the earth were suddenly lowered at any place, the flow of heat to the surface would by that means be temporarily increased, and therefore also the rate of increase near the surface. But since the general internal temperature is no greater below that locality than elsewhere, this increased flow near the surface must take place at the expense of the superficial strata. Hence the isogeotherms near the surface, though temporarily brought closer together, will shortly begin to separate again, until the rate of increase falls to its normal value. At this juncture the flow of heat at the surface at the place in question will become equalized to that which obtains elsewhere. And since the supply of heat from the interior is the same here as elsewhere, the rate can fall no lower, and will afterwards continue equal to that which obtains in other regions. It follows that the permanent rate of increase of temperature near the surface of the earth will be everywhere the same, and the flow of heat also, whatever the temperature of the surface may be. This is what observation shows to be the case.

It is evident that this result will be independent of the cause which maintains the temperature of the surface (say) below the average, whether it be a cold climate or an ice-sheet. We may therefore assume that the rate of increase in the earth beneath the ice-sheet is $\frac{1}{60}$ of a degree Fahr. per foot of descent.

Let us now suppose that our sheet of ice is throughout below the melting-temperature, so that the level of that crucial temperature is situated within the rock. In this case the flow of heat from the earth will pass into the ice unaltered in amount, because none of it will be arrested and consumed in melting ice at the junction. The ice will then be under conditions which will render it sufficiently amenable to the following statement of Fourier:—

“The thermometric state of a solid enclosed between two parallel infinite planes, whose perpendicular distance is e , and which are maintained at fixed temperatures, a and b , is represented by the two equations”

$$v = a - \frac{a-b}{e}(e-z), \quad (1)$$

$$\left. \begin{array}{l} F = k \frac{a-b}{e} \\ F = k \frac{dv}{dz} \end{array} \right\} (2)$$

where, for convenience in the present instance, we have taken the origin of z (the depth) at the surface.

v is the temperature at the depth z ,
 a „ „ of the lower surface,
 b „ „ of the upper surface,
 k the conductivity,

and F is the flow of heat upwards through the solid.

The last of these equations will be applicable to the rock if the proper value be assigned to k .

Let K be the conductivity of rock,
 k „ „ ice.

Now $\frac{dv}{dz}$ represents the rate at which the temperature increases in descending. We know that for rock this is $\frac{1}{60}$, the units being the foot and degree Fahr. Suppose β to be the value of $\frac{dv}{dz}$ for the ice; F will be, under the circumstances

supposed, the same both for the ice and rock. Hence equation (2) becomes,

$$\text{for the rock, } F = K \frac{1}{60},$$

$$\text{for the ice, } F = k\beta,$$

whence

$$\beta = \frac{K}{k} \frac{1}{60}.$$

Now the *ratio* of the conductivities will be the same whatever system of units we employ.

Referring to Professor Everett's 'Illustrations of the C.G.S. System of Units,' we find the mean of K for three kinds of rock *in situ*, as determined by Sir Wm. Thomson, to be .00581, and the mean of k for ice .00218; whence

$$\begin{aligned}\beta &= \frac{581}{218} \frac{1}{60} \\ &= .04441.\end{aligned}$$

Hence, for the ice,

$$\frac{a-b}{e} = .04441,$$

in which expression a is the temperature of the surface in contact with the rock, and b is the temperature of the surface exposed to the air, e being the thickness of the ice.

If, therefore, we wish to find the thickness of ice which will just be sufficient not to induce melting at the bottom, we must put 32 for a ; and if, with Dr. Croll, we suppose the mean temperature of the surface exposed to the air to be 0° F., we must put 0 for b . Whence

$$\frac{32}{e} = .4441;$$

$$\therefore e = 743 \text{ feet.}$$

If the thickness of the ice be less than this, no melting will take place; but if greater, there will be melting at the junction of the ice and rock.

Here no account has been taken of the lowering of the melting-point by pressure. But that is easily allowed for. For, comparing the height of a column of ice whose pressure is equivalent to a column of mercury of 30 inches (or one atmosphere), it is 37 feet.

Since, then, the melting-temperature is lowered by 0.0137° F.

for each additional atmosphere of pressure, it will be lowered

$$\frac{e}{37} \times .0137^\circ \text{ for the thickness } e \text{ of ice.}$$

Hence we shall have, to determine the limiting thickness of the ice at which its lower surface will begin to liquefy,

$$\frac{32 - \frac{e}{37} \times .0137}{e} \times .04441;$$

$$\therefore e = 714 \text{ feet.}$$

(2) *No certain limit can be imposed upon the thickness to which the ice might accumulate, provided the snowfall be more than sufficient to counterbalance the melting at the bottom.*

We have found the critical value of the thickness of the ice so that melting should just not take place. Suppose the ice thicker than this; then the bottom of it will begin to melt, and consequently must be at the melting-temperature corresponding to the pressure. The flow of heat out of the earth will melt off a layer of it annually. But the whole of this flow of heat will not be so employed, because, the ice being maintained in a condition in which its upper and lower surfaces are at different temperatures, there must ensue a flow of heat through it. This flow of heat will be expressed by the equation

$$f = k \frac{a-b}{e}.$$

Or if, as before, we assume the temperature of the upper surface of the ice to be zero, and allow for the lowering of the melting-point by pressure,

$$f = k \frac{32 - e \times .00037}{e}.$$

Now it is obvious that there is no source from which this flow can be derived, except the flow out of the earth. Hence F , which is the flow out of the earth, must be split up into two portions, $F-f$ and f ; of which $F-f$ goes to melt the ice at the bottom, while f is conducted away through the ice into space beyond. Now

$$\begin{aligned} f &= k \frac{32 - e \times .00037}{e} \\ &= k \left(\frac{32}{e} - .00037 \right). \end{aligned}$$

We see, then, that as the thickness of the ice is increased, f is diminished, and the less heat escapes through the ice; and

when the thickness exceeds sixteen miles none will escape. But if none escaped, so that the whole of F was employed in melting the ice, Sir William Thomson asserts that it could only melt one fifth of an inch annually at the ordinary temperature and pressure. If, therefore, the snow-fall considerably exceeds this small amount, there seems to be no reason why the ice might not accumulate to a much greater thickness than the above, as far as melting at the bottom is concerned.

Note.—Professor Everett says, p. 45, that Sir Wm. Thomson's results are given in terms of the foot and second, and that consequently they have been multiplied by 929 to reduce them to the C.G.S. system. Hence to bring back K and k , as given by Professor Everett, to the system of units used here, we must put

$$K = \frac{\cdot 00581}{929}$$

and

$$k = \frac{\cdot 00218}{929}.$$

The thermometric scale used does not affect the value of the conductivity.

The next question which we will attempt to answer regards

(3) *The mode of origin of the stratification of the great tabular icebergs of the south.*

Supposing that each separate stratum, distinguished by alternations of more or less clear ice, is the product of the snowfall of a single year, Sir Wyville Thomson suggests the following as the mode by which the lower strata may have lost some of their original thickness:—"It is probable that, under the pressure to which the body of ice is subjected, a constant system of melting and regelation may be taking place, the water passing down by gravitation from layer to layer until it reaches the floor of the ice-sheet, and, finally, working out channels for itself between the ice and the land, whether the latter be subaerial or submerged."

Is this process of melting and regelation possible? If we consider the ice-sheet uniform in structure and at rest, it is obvious that the pressure upon any given area of section will be greatest at the bottom, where also the temperature will be highest. Consequently, under these circumstances, the bottom is the only place where the pressure would induce melting. But it may be replied that the ice is not at rest, but is moving outwards towards its free edge. And pressures in the horizontal direction may be connected with this movement greater than the vertical pressures due to the mere depth. It must,

however, be recollected that the outward movement of the ice is not caused entirely by a horizontal pressure arising from a *vis a tergo*, but that at every place there is a tendency to flow outwards towards the unsupported free edge. In ordinary glaciers the strain thus produced occasionally gives rise to crevasses. It seems improbable, therefore, that the horizontal pressure should ever exceed the vertical; so that if the latter cannot induce melting, neither can the former do so.

It is, however, more likely that what Sir Wyville Thomson contemplated was that the pressure of one layer upon the next is not evenly distributed, owing to the layers being in contact in some places but not in others. In this case the pressures upon the areas in contact might increase to a great amount, provided the areas of contact were sufficiently small. And it is quite likely to be partly in this way that *névé* is converted into solid ice; for the contact along any horizontal plane is confined to those areas in the *névé* which are occupied by ice and not by air, so that the pressure on these surfaces is much greater than if the ice were uniformly solid. In fact, the mean pressure upon the portion of any area which is occupied by ice is to the pressure due to the depth alone as the whole of that area is to the area occupied by ice. When the conversion of *névé* into ice has taken place, it is not easy to understand how the process of melting and regelation can go further.

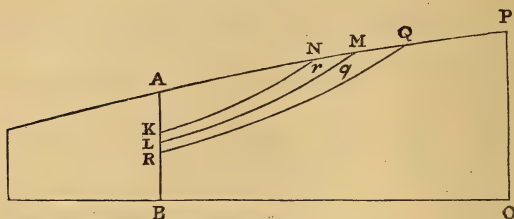
But Dr. Croll has pointed out that the diminution of the thickness of the annual strata of ice from the top downwards may be accounted for by the fact that the ice radiates from a centre of dispersion. Although I am fully aware of the very slight practical value of any addition I can make to what Dr. Croll has said upon this part of the subject (for we are too ignorant of the physical properties of ice to arrive at any certain results), I propose nevertheless to offer a few further remarks upon the mode of origination of the stratified structure, and the consequences which follow as affecting the thickness of the strata.

The snowfall of each year is deposited upon that of the previous year; and there is no internal cause to alter this order of "conformable" arrangement of the strata. It is conceivable that the movement of the ice over a rough rocky surface might dislocate the strata, or that the liquefaction of the upper surface over partial areas might cause the snowfall of certain periods to be removed before the deposition of some later strata took place, and so render the strata unconformable. But these disturbing causes are supposed not to be present. It follows that, in any vertical column, the snowfall of every year intermediate between the earliest represented in it (which will be at the

bottom) and the most recent (which will be at the top) must have its corresponding layer.

Joining the corresponding layers in contiguous columns, it appears that there must be a regular stratification, as was observed to be the case, and that the strata will not deviate far from horizontality. The question then arises, Where were these strata deposited? and what regulates their thickness?

Fig. 1.



Let a cylindrical surface AB be described in the ice-cap around the polar axis PO , with radius r approximately equal to PA . Also describe a ring on the surface of the ice, whose width is w_n ; and let ρ_n be the distance of the edge of it further from the pole. NM is a section of this ring, and $PN = \rho_n$; $NM = w_n$.

Now, if the form of the ice-cap is permanent, the snow which falls upon the ring, and upon any part of it, will always follow the same route to meet the cylindrical surface at AB . Suppose NK to be the route taken by the particles which travel from N , and ML by those from M . It is evident, then, that the ice at AB will consist of layers, each of which is continually fed by the snowfall upon its own corresponding ring fixed in position upon the surface, and that the rings nearer the pole will supply the lower layers. This conclusion is independent of any assumption as to the forms of the paths pursued by the ice. It only supposes that these paths do not cross one another.

Suppose that $AB = h$, $KL = \delta h_n$. Then, if s be the depth of the annual snowfall upon NM , we must have the quantity of ice which passes outwards each year through the cylindrical surface at KL equal to the annual snowfall on the ring at NM .

Now the area of the ring at NM is

$$\pi(\rho_n^2 - (\rho_n - w_n)^2) = \pi(2\rho_n w_n - w_n^2).$$

Taking v to represent the mean annual velocity through KL per annum, we must have, from the above consideration,

$$2\pi r v \delta h_n = \pi s (2\rho_n w_n - w_n^2).$$

$$\delta h_n = \frac{s}{vr} \left(\rho_n w_n - \frac{w_n^2}{2} \right).$$

This relation expresses the thickness of the layer of ice at A B which is derived from the snowfall on the ring whose section is N M.

If, then, δh_n be the thickness of a layer derived from one year's snowfall, or an annual stratum of ice at A B, this implies that it takes one year more for a particle to travel from M to L than from N to K.

(4) *To account for the downward diminution in thickness of the annual strata of ice.*

For the width of the ring next nearer to the pole we shall have to substitute for ρ_n the value $\rho_n - w_n$; and if the width of that ring be $w_n + \alpha$, and δh_{n-1} the thickness of the corresponding stratum at A B, taking s and v as constant for adjoining strata, we shall have

$$\delta h_{n-1} = \frac{s}{vr} \left((\rho_n - w_n)(w_n + \alpha) - \frac{(w_n + \alpha)^2}{2} \right),$$

whence it appears that

$$\delta h_n - \delta h_{n-1} = \frac{s}{vr} \left(w_n^2 + \frac{\alpha^2}{2} - \alpha(\rho_n - 2w_n) \right). \quad . \quad . \quad (A)$$

Now we do not know whether the rings which contribute the annual strata diminish or increase in width as they approach the pole. But we may gain a knowledge of the effect which a diminution or increase in the width of the rings would have upon the relative thickness of the successive strata at A B by putting $\delta h_n - \delta h_{n-1} = \beta$, and considering it as the ordinate of a curve whose abscissa is α . If we suppose A B to be near the free edge of the ice-cap, r will be large; and we may without much risk of error consider v constant for all depths above the water-level in that position.

Substituting β and suppressing the suffixes, and writing m for $\frac{vr}{s}$, which we now take as constant, and observe that it is large, because although v is small, yet r is large and s is small, we obtain

$$(\alpha - (\rho - 2w))^2 = 2m \left(\beta + \frac{(\rho - 2w)^2 - 2w^2}{2m} \right).$$

This represents a parabola whose axis is vertical, and which

has for the coordinates of its vertex,

$$\rho - 2w \text{ and } -\frac{(\rho - 2w)^2 - 2w^2}{2m}.$$

The *latus rectum* $\frac{2vr}{s}$ is independent of ρ and w , and may be taken as constant. Supposing that we draw the curve with assigned values of ρ and w , then the ordinate β to abscissa α will give the difference in thickness between the strata at A B which are derived from two contiguous rings whose widths are w and $w + \alpha$.

If we put $\alpha = 0$, then $\beta = \frac{w^2}{m}$; so that the height at which the curve cuts the axis of β is independent of ρ , except so far as w depends upon ρ . The points at which the curve cuts the axis of α are given by the relation

$$\alpha = \rho - 2w \pm \sqrt{(\rho - 2w)^2 - 2w^2}.$$

This must be always positive.

Taking the smaller value, and observing that, except near the pole, $\rho - 2w$ is much greater than w , we have, expanding,

$$\alpha = \frac{w^2}{\rho - 2w} \text{ nearly,}$$

which, except near the pole, is much smaller than w . As soon as α exceeds this value, β will become negative.

The greatest negative value of β will be attained at the vertex, where

$$\alpha = \rho - 2w.$$

In this case it will be found, by reference to the distances measured from the pole, that this value of α would carry the ring whose width is $w + \alpha$ up to the pole itself.

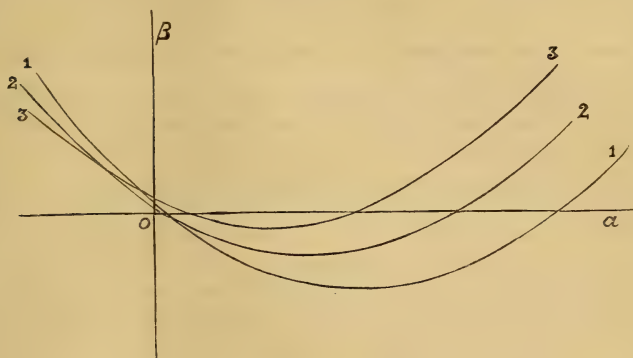
We can now perceive in a general way how a decrease or an increase in the width of the rings as they approach the pole would affect the difference in the thickness of successive strata at A B.

The *latus rectum* of the parabola being constant, the curve always maintains the same size. We have therefore only to draw it with its axis vertical, and to place the vertex in the position corresponding to the assumed values of ρ and w . It will then necessarily cut the axis of β at the height $\frac{w^2}{m}$ above the origin; for this is the value of the ordinate corresponding to $\alpha = 0$. This shows that, if the rings were of uniform width, the difference in thickness of the annual strata at A B would

be constant ; or, in other words, their thicknesses would form a decreasing arithmetical progression, whose common difference would be $\frac{w^2}{m}$.

Let curve 1 in fig. 2 be drawn with any assumed values

Fig. 2.



of ρ and α . And, first, suppose the rings to decrease in width as they approach the pole. Then the difference in thickness between the ring under consideration and the next nearer to the pole is given by the ordinate corresponding to the assumed value of α on the left of the origin. We observe these ordinates to be all positive. Hence the strata decrease in thickness.

Now take other values of ρ and w nearer to the pole. Then both ρ and w will be diminished. On account of the diminution of ρ , which we suppose much greater than w , the vertex of the parabola will be raised ; and because $\frac{w^2}{m}$ is diminished, the point at which it cuts the axis of β will be lowered (compare curves 1 and 2), and the ordinate corresponding to the former negative value of α will be less than it was before, and less still for the probably smaller value of α which corresponds to the diminished value of ρ . Hence the difference in the thickness of the strata will become less and less for the lower ones.

Next suppose the width of the rings which supply the annual strata at A B to increase as they approach the pole. Then the difference in thickness between the stratum derived from the ring under consideration and the next to it nearer to the pole is given by the ordinate corresponding to the

assumed value of α on the right of the origin, and, as before, is positive unless α is greater than $\frac{w^2}{\rho - 2w}$.

Now take other values of ρ and w nearer to the pole. Then ρ is diminished, while w is increased. On account of the diminution of ρ and increase of w , the vertex of the parabola will be brought nearer to the origin, and raised (but more so than in the previous case, when the rings diminished). And because $\frac{w^2}{m}$ is increased, the point at which the curve cuts the axis of β will be raised (compare curves 1 and 3), and the ordinate corresponding to the former positive value of α will be greater than it was before; and for a slightly greater value of α , such as we may presume belongs to the next pair of annual rings towards the pole, it will be still greater; and so on for the next position of the curve. Consequently in this case the difference between the thicknesses of the annual strata will for large values of ρ be at first small, and will become larger in descending. But should the value of α increase rapidly, as it may possibly do on approaching the pole, then the difference would begin to decrease.

But the strata which are visible above the water-level must certainly be derived from rings distant from the pole, for which the values of ρ are large. Hence, on the whole, we may conclude that in the visible ice-cliff the differences of thickness in the strata in descending (1) would be constant if the rings from which they are derived were of uniform width, (2) the difference would diminish if the rings diminished in width, and (3) it would increase if they increased in width.

It does not appear that any sufficiently close observations have been, or perhaps could be, made to determine the rate of decrease in the thickness of the successive strata. In the address referred to, Sir Wyville Thomson tells us that the ice-cliff of a berg was on an average about 200 feet high, that at about 80 feet below the top the strata were about a foot thick, and near the water-line about three inches. These data are not sufficient to warrant any conclusion beyond the mere fact of the diminution in thickness.

Let the points r and q be so taken, in fig. 1, that the time which a particle of ice takes to travel from N to K is equal to that which it takes a particle to travel from r to L; and similarly from M to L and from q to R; then, if NM and MQ supply annual strata at A B, it will take one year for a particle to travel from M to r , and the same period for one to travel from Q to q . Hence the mean velocity through M r : mean velocity through Q q :: M r : Q q .

But it may be fairly assumed that $M r$ is greater or less than $Q q$, according as $N M$ is greater or less than $M Q$. Consequently, if the rings supplying the annual strata increase in width, the velocity of the ice near the surface also increases, as the pole is approached, and *vice versâ*.

The permanence of height in the ice-cap must depend upon the fact, that the resolved part of the annual velocity near the surface in the vertical direction is equal to the depth of the annual snowfall. Consequently, for a given path, a widening of the rings corresponding to a greater velocity would be consonant with a thinner ice-cap, and a narrowing of them with a thicker one.

LXII. *Action of Light upon the Soluble Iodides, with the Outlines of a New Method in Actinometry.* By ALBERT R. LEEDS, Ph.D.*

THE question as to whether potassium iodide, in dilute solution, is decomposed by free sulphuric acid, has frequently been made a matter of controversy. Schönbein contended that it was not, and, in an acrimonious reply to Prof. Fischer (*Journ. für prakt. Chem.* 1845, xxxiv. p. 492), impugned the purity of the latter's chemicals. The same ground was taken by Houzeau, in a discussion with M. L. Sauvage (*Comptes Rendus*, 1868, lxvii. pp. 633, 714, 1138), the former going so far as to state that, when the solutions were a thousand times dilute, no decomposition took place even on protracted boiling. These discrepancies appear to have originated from an oversight of the essential part played by air or oxygen in the reaction. This is represented by the general equation $MI + HA + O = MA + H_2O + I$, where M indicates the basic and A the acid radical, coefficients being omitted. This holds true not only of the ordinary mineral acids, but has been verified in the three of the organic acids experimented upon—oxalic, tartaric, and acetic acids. *In the dark* the decomposition diminishes with the increase of dilution, being indeterminable, when the dilution has reached the one-thousandth, at the end of twelve hours, but plainly recognizable when the dilution has attained the one four-thousandth at the expiration of five days. These figures apply more especially to the potassium-iodide and sulphuric-acid solutions, upon which very numerous quantitative determinations of the liberated iodine were made. *In sunlight* the amount of iodine set free increases in the same ratio as the increase in surface of exposure to the

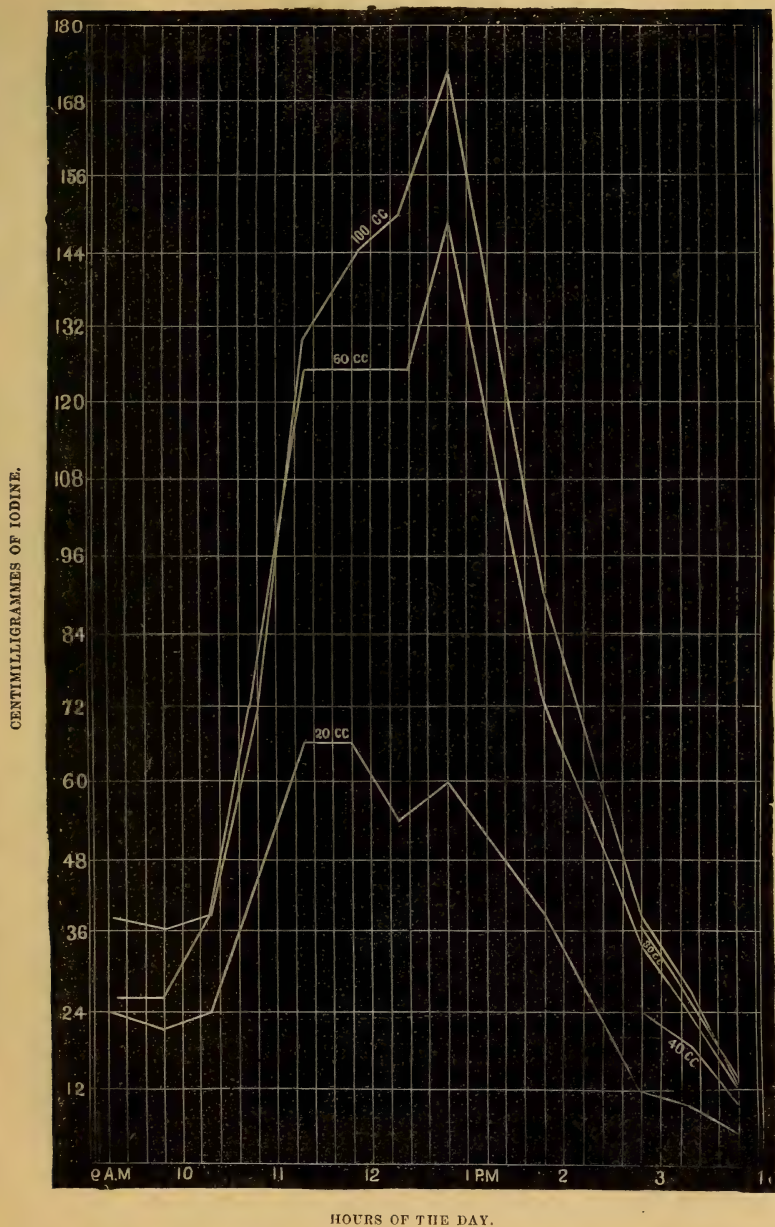
* Communicated by the Author.

sun's rays, or, in case of solutions exposed in tubes of colourless glass and of uniform bore, in the same ratio as the increase of volume, or the dilution. In the absence of air, no decomposition takes place either in light or darkness. When the air has been entirely removed by a long-continued current of carbonic acid, the solution may be exposed for days to the sun without undergoing change. If the carbonic acid be replaced by a stream of oxygen, decomposition begins, and, in the case of potassium-iodide solution a thousand times dilute and exposed to the sun, may attain to 6 mgrms. of liberated iodine per hour.

When these principles had been experimentally established, they were applied in the first instance to an actinometric measurement of the solar ray. The solutions were contained in comparison-tubes, which when filled to the depth of 150 millims. held 100 cubic centims. In each were placed 1 cubic centim. of potassium iodide of 10 per cent., 1 cubic centim. of sulphuric acid, 5 cubic centims. of starch-water, and sufficient water to make up the volume to 20, 40, 60, 80, or 100 cubic centims. They were supported on a frame kept normal to the sun's ray. The results are summarized in the accompanying diagram; and it will be noted that the curves of the more concentrated are regularly circumscribed by those corresponding to the more dilute solutions. The determination was interrupted at 2 P.M. by the sun's clouding over; but when resumed two days later, numbers were obtained which introduce no sudden breaks in their appropriate curves.

In the subsequent experiments the acids employed were some prepared with especial care by myself, and of such strength that 1 cubic centim. of the sulphuric was equivalent to 25 cubic centims. of a normal soda solution, 1 cubic centim. of the hydrochloric to 10.7 cubic centims., and 1 cubic centim. of the nitric acid to 12.6 cubic centims. of the normal soda. Similar remarks apply to the soluble iodides, of which 1 cubic centim. of a 20-per-cent. potassium-iodide solution was used in each of the subsequent trials, and 1 cubic centim. of solution of the remaining iodides, these solutions being chemically equivalent to a 10-per-cent. solution of the potassium iodide. In every case 1 cubic centim. of the acid was likewise employed, and the liquid made up to 100 cubic centims. Advantage was taken of an exceptionally brilliant day; and the actinometric measurement was repeated with the above reagents, the results being as given in the subjoined Table (p. 396). The figures in the vertical columns are milligrammes of iodine set free during each half hour of the day. It having been found that the amounts of iodine set free in the absence of starch much ex-

Action of Sun's Rays, January 22 and 24, 1879.



Actinometric Determinations, February 27, 1879.

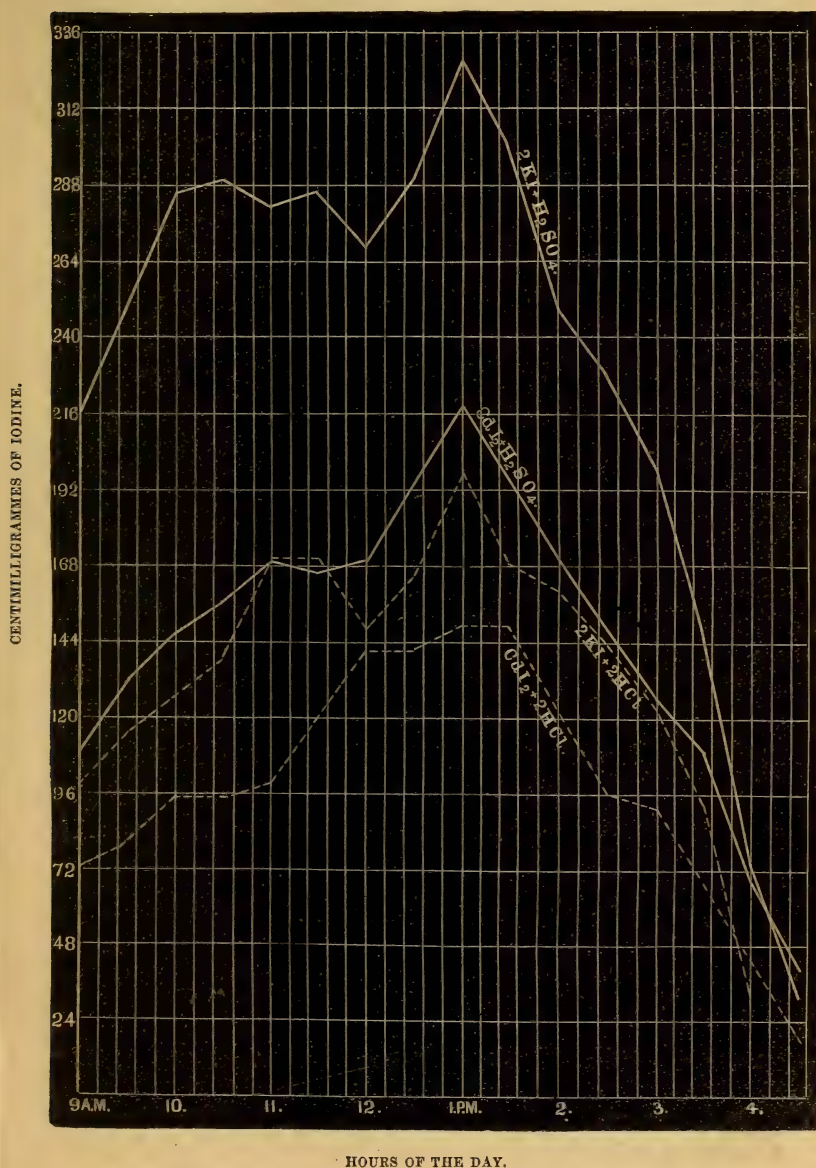
Reagents.	9 A.M.- 9.30.	9.30- 10.	10- 10.30.	10.30- 11.	11- 11.30.	11.30- 12 M.	12- 12.30.	12.30- 1 P.M.
1 c.c. H_2SO_4 + 1 c.c. KI	2.15	2.50	2.85	2.90	2.80	2.85	2.70	2.90
„ HCl „	1.00	1.15	1.25	1.40	1.70	1.70	1.55	1.65
1 c.c. H_2SO_4 + 1 c.c. CdI_2 ...	1.15	1.30	1.45	1.55	1.70	1.65	1.70	1.95
„ HCl „	0.75	0.85	1.00	1.00	1.05	1.20	1.40	1.40
1 c.c. H_2SO_4 + 1 c.c. LiI	1.20	1.30	1.35	1.45	1.60	1.50	1.60	1.70
„ HCl „	0.75	0.80	0.95	1.00	1.00	1.00	1.10	1.20
1 c.c. H_2SO_4 + 1 c.c. NH_4I ...	1.30	1.40	1.55	1.65	1.80	1.70	1.80	1.90
„ HCl „	0.85	0.95	1.00	1.05	1.15	1.15	1.15	1.20

Reagents.	1-1.30.	1.30-2.	2-2.30.	2.30-3.	3-3.30.	3.30-4.	4-4.30.	4.30-5.
1 c.c. H_2SO_4 + 1 c.c. KI	3.25	3.00	2.50	2.25	2.00	1.55	0.75	0.35
„ HCl „	2.00	1.70	1.60	1.45	1.25	0.90	0.35	0.23
1 c.c. H_2SO_4 + 1 c.c. CdI_2 ...	2.20	1.95	1.70	1.50	1.30	1.10	0.70	0.43
„ HCl „	1.50	1.50	1.20	0.95	0.90	0.65	0.35	0.20
1 c.c. H_2SO_4 + 1 c.c. LiI	1.55	1.45	1.50	1.40	1.25	1.05	0.65	0.43
„ HCl „	1.20	1.10	1.00	0.75	0.70	0.50	0.30	0.18
1 c.c. H_2SO_4 + 1 c.c. NH_4I ...	1.95	1.80	1.70	1.50	1.20	1.05	0.70	0.45
„ HCl „	1.15	1.15	1.10	0.85	0.90	0.60	0.30	0.18

ceeded those liberated when starch was present, these results, unlike those represented in the preceding diagram, were obtained with acid and iodide only, and are correspondingly greater throughout. The ratio of the iodine liberated in the potassium-iodide solution with sulphuric acid is to that liberated with hydrochloric acid as 2.33 : 1.31, in the cadmium-iodide as 1.47 : 1.0, in the lithium-iodide as 1.31 : 0.85, and in the ammonium-iodide as 1.47 : 0.92. The above results are graphically represented for two of the curves in the accompanying diagram.

An actinometric measurement was then made of the electric light, similar solutions being employed. A cylindrical stand was used, of such dimensions that the centre of the axis of the 100-cubic-centims. column contained in each comparison-tube should be at a distance of 6 inches from the focus of the electric light. The electric arc was taken between carbon points, and was produced by electricity generated with a dynamo-electric machine run by steam-power, its illuminating capacity varying during the course of the experiment, according to

photometric measurements made by Professor Morton, between 7000 and 7500 candles.



Electric Light.

	20 minutes.	7½ minutes.
1 c.c. H_2SO_4 + 1 c.c. KI	=3.90 mgrms. I.	=3.00 mgrms. I.
" HCl "	=3.15 "	=2.00 "
" "	=3.40 "	—
1 c.c. H_2SO_4 + 1 c.c. CdI_2	=3.15 "	=2.25 "
" HCl "	=2.30 "	=1.35 "
" "	=2.50 "	—
1 c.c. H_2SO_4 + 1 c.c. LiI	=3.00 "	=2.10 "
" HCl "	=1.70 "	=1.00 "
1 c.c. H_2SO_4 + 1 c.c. NH_4I	=2.50 "	=2.30 "
" HCl "	=1.50 "	=1.50 "
" HCl "	=1.85 "	—

The reason for the variation in the two experiments is not quite clear; for while the light was remarkably uniform during the second experiment, there was no such striking difference in the apparent luminous effect.

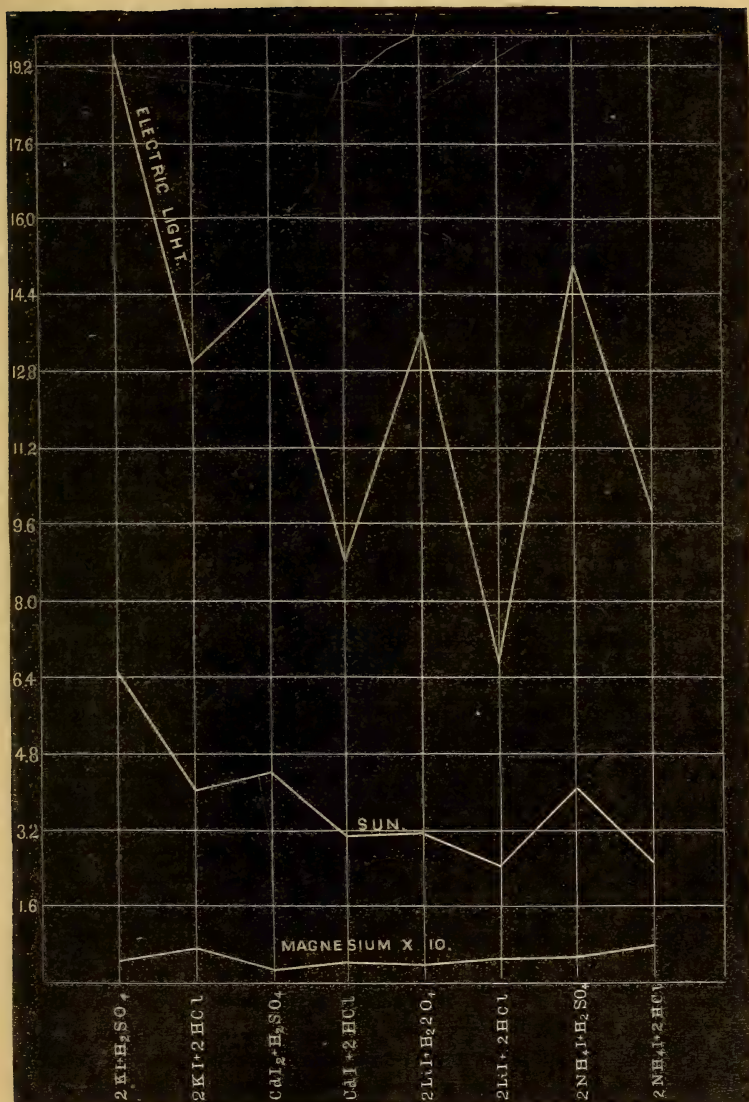
Magnesium Light.

	I.	II. (1 hour).	III. (½ hour).
	mgrm. I.	mgrm. I.	mgrm. I.
1 c.c. H_2SO_4 + 1 c.c. KI	lost	=0.050	=0.015
" HCl "	=0.225	=0.075	=0.050
" H_2SO_4 + 1 c.c. CdI_2	=0.080	=0.035	=0.010
" HCl "	=0.150	=0.050	=0.050
" H_2SO_4 + 1 c.c. LiI	=0.075	=0.040	indet.
" HCl "	=0.165	=0.050	=0.060
" H_2SO_4 + 1 c.c. NH_4I	=0.115	=0.060	—
" HCl "	=0.200	=0.070	—

The light was furnished by a single-ribbon magnesium lamp, run by clockwork. The reason for repeating the experiment *three* times was that the ratio of decomposition in presence of free hydrochloric acid was greater than in presence of free sulphuric acid. When the sun and electric light were employed, it was always less, the reagents being of the strengths above given. It will be necessary to determine the rate of decomposition in each case, for the various regions of the spectra of the three lights, before venturing upon an explanation of these differences.

The foregoing results are brought together in the following Table, and likewise illustrated in the diagram. The numbers found for the magnesium light have been multiplied by ten, in order to bring the corresponding curve into the same illustration with the others.

MILLIGRAMMES OF IODINE.



Decomposition during equal times (1 hour).

	Electric light.	Sun.	Magnesium.
$\text{H}_2\text{SO}_4 + \text{KI}$	= 19·5 mgrms. I.	6·5 mgrms. I.	0·05 mgr. I.
HCl "	= 13·0 "	4·0 "	0·075 "
$\text{H}_2\text{SO}_4 + \text{CdI}_2$	= 14·6 "	4·4 "	0·035 "
HCl "	= 8·8 "	3·0 "	0·050 "
$\text{H}_2\text{SO}_4 + \text{LiI}$	= 13·7 "	3·1 "	0·04 "
HCl "	= 6·5 "	2·4 "	0·05 "
$\text{H}_2\text{SO}_4 + \text{NH}_4\text{I}$	= 15·0 "	3·9 "	0·06 "
HCl "	= 9·8 "	2·3 "	0·07 "

As yet no actinometric measurements of spectra have been made by this method. As a preliminary determination, however, the amount of action upon the soluble iodides, after the light of the sun and electric arc have traversed certain absorbing media, has been studied. The comparison-tubes were supported in the centres of tall wide-mouth bottles, so as to be surrounded by an equal thickness (3 centims.) of the absorbing medium on every side. Their tops and those of the bottles were closed in with tinfoil, so as entirely to prevent access of light, but admit air. The blue medium was ammoniacal solution of copper; the yellow, neutral potassium chromate; the red, fuchsine. Their strength was adjusted to the point of equal translucency.

Absorbing Media (time 1 hour).

		Sunlight. mgrms. I.	Electric light. mgrms. I.
Blue.	{ 1 c.c. H_2SO_4 + 1 c.c. KI	= 2·38	= 6·00
	" HCl "	= 2·22	= 4·50
Yellow.	{ 1 c.c. H_2SO_4 + 1 c.c. KI	= 0·67	= 0·50
	" HCl "	= 0·45	= 0·45
Red.	{ 1 c.c. H_2SO_4 + 1 c.c. KI	= 1·53	= 4·95
	" HCl "	= 0·83	= 3·00

While in the preceding experiments the action in the electric light has been approximately the maximum of that occurring in sunlight, this ratio, when absorbing media were used, was approached only in case of the blue and red solutions. In other words, the yellow medium absorbed a much larger relative proportion of the actinic rays of the electric light than of those of the sun.

Stevens Institute of Technology,
Hoboken, N.J., April 15, 1879.

LXIII. *A new Theory of Terrestrial Magnetism.*
 By Professors JOHN PERRY and W. E. AYRTON*.

IN the autumn of 1876, while experimenting on magnetic transparency, we designed an apparatus for testing whether a moving body having a definite electric charge would, like a current, deflect a magnet. While waiting for the conclusion of the rains, and the advent of the very dry season which accompanies a Japanese winter, in order to try our instrument in conjunction with an ordinary plate-glass electrical machine, we received the account, published in the *Philosophical Magazine* for September 1876, of the experiments just performed by Mr. Rowland in the laboratory of Professor Helmholtz, by which it had been conclusively shown that a charge of electricity mechanically moved had the properties of an ordinary electric current as far as the deflection of a magnet was concerned†.

Until this point was settled, it was unlikely that attention would be directed to the electromagnetic effects that might arise from the rotation of a charged body like the earth. Shortly, however, after the execution of the experiments referred to we attempted (as described in our paper "On Rain Clouds and Atmospheric Electricity," which appeared in the *Philosophical Magazine* for March 1878) the solution of a new theory of terrestrial magnetism. This problem we have attacked in a variety of ways; and the following solution, to which we have at length been led, we beg to offer for the acceptance of the Physical Society.

The points near the surface of the earth have different linear velocities from those in the interior (although all the points have the same angular velocity of rotation round the earth's axis); therefore, if the earth had an initial electrical charge, residing of course, in accordance with the well-known electrical law, on its surface, the electrified particles would have velocities relative to the remainder; hence, as a direct consequence of the results of the experiments published by Professor Helmholtz, the interior of the earth would be a magnetic field, quite

* Communicated by the Physical Society, having been read at the Meeting on March 8.

† Additional confirmation has recently been given on this subject by the experiments described by Mr. Crookes in his paper on the "Illumination of the Lines of Molecular Pressure &c.," since he has shown that the stream of particles which is shot off from the negative terminal in a very perfect vacuum, and which produces the green phosphorescence, carries electricity with it and is deflected by a magnet. This may be regarded as a sort of converse experiment, since it proves that a magnet deflects a moving charge of electricity.

independently of its interior constitution. And precisely similar reasoning, of course, proves that outside the earth's surface there would also be a magnetic field. (*Vide* addition at the end of the paper.) To determine the strength of this field we have the following relationship to start with.

In § 526, Clerk Maxwell's 'Electricity,' it is shown that an element of current C , of length ST , acts upon a unit magnetic pole at a point P with a force

$$C \frac{ST}{PS^2} \sin PST,$$

in a direction at right angles to PS and ST . Combining this with the experiments referred to above, we may assume that if a charge of static electricity (measured in electromagnetic units) Q , at the point S , moves in a direction ST with a velocity v relative to a point P , it produces on a unit magnetic pole at P a force

$$\frac{Qv}{PS^2} \sin PST,$$

in a direction at right angles to PS and ST ; and this is the *only assumption* employed in the following investigation.

Now suppose the earth to have the uniform density of electricity σ over its surface, and let its radius be unity. Consider the force produced, by the rotation of the electricity at a point S on the surface having coordinates r, θ, ϕ , at a point inside the sphere having the coordinates r_1, θ_1, ϕ_1 . Then if the sphere be rotating with an angular velocity w round the axis of z , and if θ be the angle between this axis and a radius, while ϕ is the angle between the axis of x and the projection of a radius, the velocity of S relative to P will have for its coordinates

$$u \text{ or } -w (\sin \theta \sin \phi - r \sin \theta_1 \sin \phi_1) \text{ parallel to } x,$$

$$t \text{ or } w (\sin \theta \cos \phi - r \sin \theta_1 \cos \phi_1) \text{ parallel to } y;$$

also

$$PS^2 = l^2 + m^2 + n^2,$$

where

$$l = \sin \theta \cos \phi - r \sin \theta_1 \cos \phi_1,$$

$$m = \sin \theta \sin \phi - r \sin \theta_1 \sin \phi_1,$$

$$n = \cos \theta - r \cos \theta_1.$$

Now the direction-cosines of PS are proportional to l, m , and n ; and the direction-cosines of ST , the direction of motion of S relative to P , are proportional to u, v , or to $-wm, wl$, and 0 . Consequently PS is perpendicular to ST . Also, if λ ,

μ, ν are the direction-cosines of a line at right angles to PS and ST,

$$\lambda = \frac{nl}{PS\sqrt{l^2 + m^2}},$$

$$\mu = \frac{mn}{PS\sqrt{l^2 + m^2}},$$

$$\nu = \frac{-(m^2 + l^2)}{PS\sqrt{l^2 + m^2}},$$

where the negative sign must be given to the root. Now if F is the force at P due to the charge of surface-density σ on the elementary area $d\theta \cdot \sin \theta \cdot d\phi$ at S moving relatively to P, we know that it must be at right angles to PS and ST, and equal to

$$\frac{\sigma \sin \theta \cdot d\theta \cdot d\phi \times \sin PST \sqrt{u^2 + t^2}}{PS^2}.$$

Hence, if $\delta X, \delta Y, \delta Z$ be the resolved portions of the force F parallel to the axes of coordinates, and if dS stands for $\sin \theta \cdot d\theta \cdot d\phi$, we have

$$\delta X = \frac{\sigma \cdot dS \cdot \sqrt{u^2 + t^2}}{PS^2} \lambda;$$

and similarly for $\delta Y, \delta Z$.

Hence, if \iiint stands for the summation over the whole surface of the sphere, and if X is the total force at P in the direction of the axis of x ,

$$\begin{aligned} X &= - \iiint \frac{\sigma dS w \sqrt{l^2 + m^2}}{PS^2} \frac{nl}{PS\sqrt{l^2 + m^2}} \\ &= - \iiint \frac{\sigma w dS}{PS^2} \frac{l}{PS}. \end{aligned}$$

Similarly,

$$\begin{aligned} Y &= - \iiint \frac{\sigma w dS}{PS^2} \frac{m}{PS} \\ Z &= \iiint \frac{\sigma w dS}{PS^2} \frac{m^2 + l^2}{n} \frac{n}{PS}. \end{aligned}$$

Now the resolved part of the force along the axis of x is the same as would be the force in that direction due to a distribution of attracting matter of density $-\sigma w$ over the surface of the sphere. Similarly the resolved part of the force along the axis of y is the same as would be the force in that direction due to a distribution of attracting matter of density $-\sigma w$ over

the surface of the sphere; and the force in the direction z is the same as would be the force in the same direction due to a distribution having a density $\sigma w \frac{l^2 + m^2}{n}$ or $\sigma w \left(\frac{PS^2}{n} - n \right)$.

Regarding the force as being due to such a distribution,

$$Z = \iint \frac{\sigma w dS}{PS^2} \cdot \frac{PS^2}{n} \cdot \frac{n}{PS} - \iint \frac{\sigma w n dS}{PS^2} \cdot \frac{n}{PS}.$$

Now the first integral is $\iint \frac{\sigma w dS}{PS}$, which we know has a value equal to the potential inside the sphere due to a uniform distribution of density σw over the surface, and is therefore a constant, $4\pi\sigma w$. So that the entire force in the direction z is $4\pi\sigma w$ minus the force in direction z due to a distribution of attracting matter of density $n\sigma w$ over the surface of the sphere.

Now it is easy to show that a distribution of attracting matter of a density proportional to n or to $A + C \cos \theta$ over the surface of a sphere will give

$$X=0,$$

$$Y=0,$$

$$Z = \text{a constant};$$

therefore all that is necessary is to determine the value of this constant. We neglect the term A , because a uniform distribution produces a constant potential, or a zero force in all directions; the distribution $C \cos \theta$ being a zonal harmonic, produces a potential inside the earth,

$$V = \frac{4\pi C}{3} r \cos \theta_1,$$

$$= \frac{4\pi}{3} Cz;$$

so that the force Z which equals $\frac{dV}{dz}$ is $\frac{4\pi}{3} C$. Thus for the

distribution $C \cos \theta$ we have the force $\frac{4\pi}{3} C$; so that the constant force above mentioned, $4\pi\sigma w$, requires the distribution $3\sigma w \cos \theta$. From this we must subtract the distribution $n\sigma w$, or $\sigma w (\cos \theta - r \cos \theta_1)$, giving us for the total distribution of attracting matter over the surface of the sphere a density

$$2\sigma w \cos \theta + \sigma w r \cos \theta_1;$$

but the latter term means a uniform distribution, producing therefore no internal force, and may therefore be neglected. And the first term is a zonal spherical surface harmonic; there-

fore the electromagnetic potential due to the rotation of the electricity on the surface of the earth is

$$\frac{4\pi}{3} 2\sigma w r \cos \theta_1 \text{ inside the earth,}$$

and

$$\frac{4\pi}{3} 2\sigma w \frac{1}{r^2} \cos \theta_1 \text{ outside the earth,}$$

where w is the angular velocity of the earth on its axis, r the radial distance of any point from the earth's centre, θ_1 the colatitude of the place, $4\pi\sigma$ the total quantity of electricity uniformly distributed over the surface of the earth measured electromagnetically, and the unit of length the earth's radius.

These results, which we think are logical consequences of the experiment performed in Professor Helmholtz's laboratory, and referred to at the commencement of this paper, may now be applied in various ways.

For example, if the iron of the earth is arranged nearly in a hollow sphere, of external and internal radius a_2 and a_1 , then, since the potential given above is a zonal harmonic, we can at once apply Poisson's result; and we find that the potential due to magnetization of the hollow sphere is

$$\frac{4\pi\kappa(3+8\pi\kappa)(a_2^3-a_1^3)\frac{8\pi}{3}\sigma w a_2^3 \cos \theta}{9+36\pi\kappa+32\pi^2\kappa^2(a_2^3-a_1^3)} \frac{1}{r^2}$$

for all points outside the outer surface of the sphere; and hence, for points outside the surface of the earth, the total magnetic potential is

$$V = \left[\frac{4\pi\kappa(3+8\pi\kappa)(a_2^3-a_1^3)\frac{8\pi}{3}\sigma w a_2^3}{9+36\pi\kappa+32\pi^2\kappa^2(a_2^3-a_1^3)} + \frac{8\pi\sigma w}{3} \right] \frac{\cos \theta}{r^2},$$

where κ is the coefficient of magnetization.

Now Biot's approximation to the law of intensity of the force is

$$\sqrt{1+3\sin^2\lambda},$$

where λ is the latitude of the place; and we understand that this approximation is generally considered, for rough purposes, as a fairly accurate one.

Our equation for any point at a distance r from the centre and having a colatitude θ is

$$V = M \frac{\cos \theta}{r^2};$$

and

$-\frac{dV}{d\theta}$ is the force directed towards the north,

$-\frac{dV}{dr}$ is the force directed downwards towards the earth's centre ;

therefore, if I be the magnetic intensity,

$$I^2 = \left(\frac{dV}{d\theta}\right)^2 + \left(\frac{dV}{dr}\right)^2 \\ = \frac{M^2 \sin^2 \theta}{r^4} + \frac{4M^2 \cos^2 \theta}{r^6},$$

or, putting r equal to unity (that is, for a point on the earth's surface),

$$I = M\sqrt{1 + 3 \cos^2 \theta} \\ = M\sqrt{1 + 3 \sin^2 \lambda},$$

which is Biot's expression.

Now this is a result which could not have been anticipated, and speaks well for our new theory of terrestrial magnetism.

It is well known that many forms of distribution of iron inside the earth may be found which, with the existing potential given above, will produce Gauss's distribution of potential over the surface of the earth ; and it would be very interesting to try how close an approximation to the real potential would be obtained by considering the iron of the earth to form a hollow ellipsoid, one of its principal axes coinciding nearly, but not quite, with the earth's axis. This calculation would be comparatively easy ; but we prefer at present merely confining ourselves to simple illustrations of our theory.

Thus, let us, for simplicity, assume that the magnetic matter of the earth is iron, with everywhere a coefficient of magnetization

$$\kappa = 30^* ;$$

then the terms involving κ^2 will be large compared with the others ; so that if, for a very rough approximation, we assume a_1 to be nought, and a_2 equal to unity, or the whole earth to consist of iron, we find

$$V = \frac{16\pi}{3} \sigma w \frac{\cos \theta}{r^2}.$$

* κ for ordinary iron is probably between 20 and 30 ; and in our ignorance of the internal state of the earth, or what effect the great heat or pressure may have on the coefficient of magnetization, we are compelled to use this value ; but it is possible, of course, that the real value of κ may be very different.

Now Gauss gives for the magnetic moment of the earth,

$$3\cdot3092 n^3,$$

in millimetre-milligramme-second units, and where n is the number of centimetres in the earth's radius. Consequently, since the dimensions of a magnetic moment are

$$M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-1},$$

the earth's magnetic moment becomes

$$\cdot00033092 n^{-\frac{5}{2}} n^3,$$

the units being the earth's radius, gramme, second.

Assuming Biot's distribution of magnetic force over the surface of the earth, which is also what our theory has led us to, we then get from Gauss's expression for the moment the result that the magnetic potential on the earth is

$$0\cdot33092 \cos \theta n^{-\frac{1}{2}},$$

or

$$0\cdot00001311 \cos \theta \text{ nearly ;}$$

so that roughly we have, for a point on the earth's surface,

$$\frac{16\pi}{3} \sigma w = 0\cdot00001311.$$

But

$$w = \frac{2\pi}{24 \times 60 \times 60} ;$$

\therefore the density

$$\sigma = 0\cdot0107 \text{ unit of electricity,}$$

or the total charge

$$= 4\pi \times 0\cdot0107,$$

the fundamental units of space, mass, time being the radius of the earth, the gramme, and the second. But the dimensions of σ are $M^{\frac{1}{2}} L^{\frac{3}{2}}$; so that, in order to express σ in C.G.S. units, we must multiply by \sqrt{n} ; therefore the total charge

$$\begin{aligned} &= 4\pi \times 0\cdot0107 \sqrt{\frac{2,000,000,000}{\pi}} \text{ C.G.S. units} \\ &= 4\pi \times 0\cdot0107 \sqrt{\frac{2,000,000,000}{\pi}} \times 10^7 \text{ microfarads.} \end{aligned}$$

To get an idea of the electromotive force required to produce this charge, let us imagine one pole of a Daniell's battery connected with the earth and the other with all bodies in space. Then, since the capacity of the earth is 630 microfarads, this charge will be produced for each cell so employed; so that, if

x is the number of cells necessary to produce our required distribution,

$$x = \frac{4\pi \times 0.0107 \times \sqrt{\frac{2 \times 10^9}{\pi}} 10^7}{630}$$

$$= 54 \text{ million roughly.}$$

We have therefore proved that if the earth be electrified, it must, from its very rotation, quite independently of all other bodies in the universe, be magnetic; and if it consist of a shell of iron, thick or thin, then that the law of distribution of magnetism produced by this electric charge in mechanical rotation will be identically that given by Biot; and, lastly, if the earth were wholly of iron, a difference of potentials of about fifty-four million volts between it and space would be sufficient to produce the necessary amount of charge.

Now, although fifty-four million volts is a large difference of potentials to be produced with a galvanic battery, there would not be the slightest difficulty in the earth having such a difference of potentials between it and space, seeing that the earth is surrounded by millions of miles of interplanetary vacuum, every inch of which is as good or better an insulator than a Crookes's vacuum; and it has been experimentally shown that many thousands of cells will not cause a discharge across even a comparatively thin film of such a vacuum.

But even without considering the highly insulating character of interplanetary space, we see from the experiments of Drs. De La Rue and Hugo Müller that the electromotive force of fifty-four million cells cannot, in all probability, initiate a spark between two points in ordinary air unless the distance separating them be less than four hundred feet. Consequently, if the electric charge which by its mechanical rotation produces the magnetism be on the earth itself and not in the air, it could not be discharged by sparking, unless another planet, having at least a potential nought relatively to the earth, came at least to within four hundred feet of its surface.

Next, as regards the sign of the electric charge on the earth's surface required to produce the earth's magnetic polarity, is it in accordance with the known phenomena of atmospheric electricity? To produce the earth's magnetism, we must have, in accordance with the known laws of electromagnetism, a *negative* current flowing from west to east, or in the direction of rotation of the earth. In the language of the new theory, therefore, the surface of the earth must be *negatively* charged; but Sir William Thomson has proved, by

observations with his electrometer, that *all* the phenomena brought to light by atmospheric electricity, on a fine day, could be produced by the sole agency of the earth having a *negative* charge and without any charge in the air itself. The negative charge, therefore, required for our explanation of the cause of terrestrial magnetism is sufficient to account for all the ordinary phenomena of atmospheric electricity.

In the preceding investigation we have supposed the electric charge to be uniformly distributed over the earth, and so have arrived at a law of magnetic intensity merely varying with the latitude. But the sun and other members of the solar system may very likely have potentials so different from that of the earth that we can hardly conceive the amounts; consequently we should expect the static electric distribution of the earth would undergo periodic changes corresponding in time with those of the ocean-tides. But alteration in the static distribution of electricity on the earth's surface means, as we have shown, alteration in the law of magnetic intensity; consequently we should expect that this magnetic intensity would vary somewhat as do the ocean-tides; and this is known to be the case. But it is also evident that, besides these regular changes, every time a great mass of vapour is suddenly formed and condensed on the earth, and whenever great changes are occurring in the solar atmosphere whereby the lines of *electrostatic induction* from the sun to the earth are altered, we should find corresponding changes in terrestrial magnetism such as we now know as magnetic storms. And not only this, but as the planets are charged bodies, their motions relatively to the sun ought to cause motions in the sun's atmosphere such that, for instance, the allineation of a number of planets and the sun, or the near approach of any planets, if the aligned or approached planets have potentials nearer that of the sun than many of the other bodies of the solar system, ought to diminish the storms in the solar envelope, and ought to alter the electrostatic distribution on the earth. But it has been shown that the near approach of a planet to the sun both affects the sun's spots and terrestrial magnetism.

And, lastly, since the iron in the earth may, from its great pressure, possess great coercive force, we should expect (as we know to be the case) that magnetic changes would lag behind the astronomical influences accompanying them.

Addition, April 17th.—Since the reading of this paper before the Physical Society, several criticisms have appeared of this proposed explanation of terrestrial magnetism. Some of

the writers have failed to realize that the various parts of a rotating sphere have relative motions one to the other, of such a nature that if some were electrified the others would become magnetized. Their difficulty seems to have arisen from the fact that the motion of a particle of a rotating rigid sphere consists of a rotation round the axis of the sphere combined with a rotation of the particle round its own axis; so that if two particles be looking at one another in one position of the sphere, they are looking at one another in all positions, just as the same side of the moon is always turned to the earth. But if this rotation of the particles round their own axes could be stopped, if, in fact, the motion of the particles became what is commonly known as "sun-and-planet motion" similar to that of the bobbins in the machinery used in sheathing telegraph-cables to prevent torsional strain being put into the iron wires as they are lapped on the core), then probably these writers would have no difficulty in seeing that the interior particles would be magnetized by the more rapidly moving electrified ones. Now the motion of an *unelectrified* particle round its own axis cannot in any way prevent it becoming magnetized by electrified particles revolving round it; for if it could, it would be equivalent to saying that, if the electrified particles were at rest and the *unelectrified* one revolving, the mere revolution of the latter would magnetize itself oppositely to the way it would be magnetized if it were at rest and the electrified ones only in motion—a result not only without experimental proof, but one also highly improbable. Consequently, if the particle has the two motions together (as it has in a rigid revolving sphere), it will still become magnetized if the surface of the sphere is electrified.

In fact, so little can the motion of a mass of iron prevent its becoming magnetized by a moving charge of electricity, that it has been suggested to us, within the last few days, by Mr. G. F. Fitzgerald, of Trinity College, Dublin, that probably a mass of iron would become magnetized by a static charge of electricity if both had rapid absolute motion in space, even although in the same direction in parallel lines and with the same velocity—in fact, that *relative* motion is unnecessary. If this assumption (which has not yet been experimentally tested) be true, then not only will the mathematical analysis required in the investigation of the problem contained in this paper be much simplified, but, in addition the charge of electricity on the earth's surface necessary to produce, by its mechanical rotation, the earth's known magnetic moment will be considerably less even than the charge calculated above. We hope to put this idea of Mr. Fitzgerald's shortly to an experi-

mental test; but in the meantime we give no further indication of the reasoning by which it has been arrived at, nor of the way in which our equations would be modified, preferring to leave the investigation in a perfectly rigid form as it now stands, rather than to introduce any assumption which might appear problematical, even although such an introduction would both add weight to our theory, and might explain, from the velocity *in space* of a place at midnight being greater than at midday, the cause of the solar-diurnal magnetic variation.

LXIV. *The Maintenance of Constant Pressures and Temperatures.* By FREDERICK D. BROWN, B.Sc.*

[Plate XIII.]

THE great majority of the results obtained from physical experiments vary with the temperature at which the observations are made. The measurements of the density of a substance, for example, of its refractive index, of its electric conductivity, of its elasticity, of the maximum tension, and of the latent heat of its vapour, all require that the temperature should not vary during the observations. Hitherto many of these measurements have been confined to temperatures differing little from that of the atmosphere; such temperatures are easily maintained constant by means of a bath of water or other liquid; but when we try to make observations at higher temperatures, the means at our disposal fail us, and we find that, except at certain points (such as 100°), we cannot keep up the same temperature long enough to make at leisure accurate readings of our instruments.

Many attempts have been made to obviate this difficulty, but, as it seems to me, without complete success. The ordinary method has been to use a large quantity of water or other liquid, and to keep it in continual agitation; above 50° , however, the temperature of such a bath is rarely, or never, rigorously constant, while the inconvenience and waste of time incurred in heating the large mass of liquid to the required point are by no means to be neglected.

In order to keep such a bath at a constant temperature, a large number of gas-regulators have been invented. In most of these a small vessel containing mercury or air is placed in the bath, and arrangements are made by which the gas-supply is partially shut off when the mercury expands beyond a certain point. Probably the most sensitive form of this kind of apparatus is that recently described by M. Benoit to the French

* Communicated by the Physical Society.

Physical Society. Here a small closed vessel containing methyl acetate is placed in the water the temperature of which is to be maintained constant; this vessel communicates with a manometer containing mercury; and as the vapour-tension of the methyl acetate increases, the mercury in the manometer rises, obstructing the flow of gas to the burner in the usual way.

I have applied this form of thermostat to a bath of water used for heating a long column of mercury. The water is contained in a vertical copper cylinder 42 inches in length by 6 in diameter; the cylinder is packed with felt on the outside, and contains within it a second smaller and shorter one open at both ends, and extending to within about 2 inches of the top and bottom. By means of a suitable mechanical arrangement, the water is made to flow continuously down the space between the two cylinders and up the inner cylinder; the upper and lower portions of the bath are thus kept at exactly the same temperature. The water is heated by allowing it to circulate through a copper coil placed over a gas-burner. Even when the small vessel of the thermostat is filled with ether, the tension of which varies much more for a given variation of temperature than does that of methyl acetate, the temperature is subject to fluctuations of as much as $0^{\circ}\cdot 1$, and this independently of the change which necessarily occurs when the pressure of the gas is altered.

Gas-regulators are employed perhaps more successfully where the gas-flame can be applied directly underneath the bath; for the rise of temperature in the water then follows more rapidly upon the increase of the flame, the flow of gas is sooner checked by the mercury, and the tendency to allow too much heat to be communicated to the water is thereby lessened. Still better results are obtained if the thermostat be applied to an outer jacket of water surrounding the bath in which the observations are made. Both these conditions, however, are generally very difficult to carry out where large quantities of water are of necessity used. Even when all these precautions are taken, a constant temperature, in the true meaning of the words, is not attained. For a further discussion of the defects of this form of thermostat see Laspeyres*.

The only practicable way of attaining the object in view appears to be afforded by the fact that the vapour emitted by a boiling liquid does not vary in temperature, provided that there is no variation either in the composition of the liquid or in the pressure to which it is subjected. Taking their stand on this consideration, Laspeyres (*loc. cit.*) and Sprengel

* Pogg. *Ann.* clii. p. 132.

(Journ. Chem. Soc. 1873, p. 458) suggested the use of mixtures of sulphuric acid and water of different strengths, which, if the vapour given off is condensed and returned to the mass, boil constantly at certain given temperatures: here the constant temperature is afforded by the liquid and not by the vapour, which is less hot; hence, if the liquid tends to become superheated, fluctuations in temperature will infallibly occur. The great objection to this method is that, when a series of temperatures is required (as, for instance, in the comparison of thermometers), great inconvenience and loss of time is incurred by having frequently to replace the liquid in the apparatus by another containing a different proportion of sulphuric acid or other substance.

A series of temperatures can be obtained with one liquid only, if the pressure under which it boils be varied. This method, simple as it appears, is beset with considerable mechanical difficulties, to surmount which the apparatus described below has been constructed.

At first sight it would seem that, if the vessel containing the steam be connected with a large closed vessel serving as air-reservoir, all that is necessary is to rarefy or compress the air in the reservoir to the required extent, and to allow the liquid to boil undisturbed. But the steam-bath is necessarily large; and the reservoir therefore must be large also. Now the reservoir must not only be strong enough to stand a vacuum inside it, but must also be capable of supporting an interior pressure of at least 50 lbs. on the square inch; such a reservoir is not only cumbrous and expensive, but dangerous. Further, any change of temperature in the room alters the pressure inside this reservoir; it must therefore be placed in a cistern of water, which is a second objection. The great obstacle to the employment of this method, however, is the difficulty of preventing leakage even with the greatest possible care. In endeavouring to stop leaks I expended so much time that I gave up the reservoir, and determined to construct an apparatus for maintaining a constant pressure in a given vessel even when it leaked.

Lothar Meyer (*Ann. Chem. Pharm.* vol. clxv. p. 303) had already devised an apparatus of this kind, adapted chiefly to fractional distillation under reduced pressure. It consists essentially of two vertical tubes, A B and C D (fig. 1, Pl. XIII.), connected at the upper part by the lateral tubes E and F; at B an india-rubber tube connects A B with the tube K, which slides up and down the board to which the whole is fixed; at the top of A B is a tube H which is connected with an air-pump; the top of C D is provided with a cork, through which a narrow tube S S passes nearly to the bottom of C D; finally, the lateral tube *k* is

connected with the apparatus X, in which a constant pressure is kept up. Sufficient mercury is poured into R to fill A B up to the lower end of H when R is about half full; C D is also filled with mercury, which is let out by the tap M until the column P S above the lower end of S S is equal to the difference between the required pressure and that of the atmosphere. If now the air-pump be set to work, a partial vacuum will be created in X and in the tubes A B and C D, the mercury will rise in A B until it touches the lower end of H; R is then placed in such a position that the vertical distance between the lower end of H and the surface of the mercury in R is equal to P S. It is now evident that when the desired pressure is reached the mercury will close up the orifice of H, thus stopping the withdrawal of air; while if the pressure is less than that required, more air will enter through the tube S S and bubble up through the mercury, and thus a more or less constant pressure will be maintained in X.

This apparatus suffers from two defects: first, the splashing of the mercury as it is sucked up H and then falls down again, together with the bubbling of air up C D, renders the pressure in X slightly variable; secondly, it is only adapted for pressures less than that of the atmosphere.

Meyer's instrument has very recently been modified by Dr. Otto Schumann and by W. Stadel and E. Hahn (*Ann. Chem. Pharm.* vol. cxcv. p. 218); the new form, although capable of regulating pressures above an atmosphere, apparently with tolerable accuracy, has a very limited range.

The apparatus for maintaining constant pressures, which I now wish to bring to the notice of the Society, consists of a manometer connected with which is an automatic arrangement for governing the supply of air. The manometer (fig. 2) consists of a tube A B having the form and dimensions shown in the figure; the lateral tube C is connected with the vessel X, in which a constant pressure is to be kept up; the tube D is connected with an air-pump or other contrivance for rarefying and compressing air. The upper end of the tube A B is closed by an india-rubber stopper, or, better, by a metal cap, through which the rod E_1 passes air-tight; this rod is tipped with platinum at its lower end. The lower end of A B is joined by means of india-rubber tube to the tube F, of which the upper part has the same diameter as that of A B; this tube is fitted with another cap and iron rod E_2 similar to the first; but the cap does not fit air-tight. The piece of wood which carries F moves along the scale S S in a groove in the board to which the whole is fixed. Lastly, the two rods E_1 , E_2 are furnished with binding-screws for copper wire, while a third binding-

screw K is connected with a small piece of steel tube interposed between the end of AB and the india-rubber tube. This third connexion with the mercury contained in the manometer may of course be made by means of a third (insulated) rod passing down to the bottom of the wider part of AB or F.

To fill and adjust the manometer, the tube F is raised to the same level as A, and mercury is poured in until it just reaches the wide cylindrical portions of A and F; the rods E_1 , E_2 are then moved until the point of E_1 just touches the surface of the mercury, while that of E_2 is a fraction of a millimetre above it.

Suppose now that F be lowered n millims. and the air-pump be set to work to pump air out at D; further, that K be connected with one pole P of a battery, and the rods E_1 , E_2 with the other pole N; then it is evident that as long as the pressure in A, and consequently in X, is greater than $H-n$ (where H = the barometric pressure), the current will pass through the circuit P K E_2 N, whereas when the pressure is less than $H-n$ the current will pass along P K E_1 N. Similarly, if F be raised n millims. above A and the pump be made to compress air into A, as long as the pressure is less than $H+n$, the current will pass along P K E_1 N; but when the pressure becomes greater than $H+n$, the current will pass along P K E_2 N. In order to economize space, the tube AB is widened out at B; and when it is intended to obtain a pressure greater than H , the rod E_1 is replaced by a long one reaching down to B; this, of course, is equivalent to lowering A or raising F nearly the whole length of the scale SS. In the further description I shall only consider the case in which F is lower than A—that is, when a pressure less than H is required; the alteration needed for higher pressures will readily suggest itself.

The apparatus depicted in fig. 3 consists of a brass tap T and an electromagnetic clutch to work the tap automatically. The tap, which is shown in section in fig. 4, is placed between the tube D of the manometer (fig. 2) and the air-pump, L_1 being connected with D and L_2 with the pump. From the figure it is readily seen that when the tap is in the position drawn, and the lever fitted to the head of the key lies in the direction ab , air will be admitted into the manometer; if, on the other hand, the lever and key occupy the position $a'b'$, the pump will withdraw air from the manometer. The object of the clutch, therefore, is to place the tap in the first position when the pressure is too small, and in the second when it is too great. The lever ab (fig. 3) terminates in two arcs which are grooved to hold a cord; these arcs are furnished with set screws S_1, S_2 , by means of which

the amount which the tap can open may be regulated. The two ends of the lever are connected by strings at S_1 S_2 to the loose pulleys P_1 P_2 of the clutch. These pulleys are made of soft iron, and run on a spindle which revolves on the centres A_1 , A_2 . Revolving with the spindle and facing P_1 and P_2 are two small electromagnets M_1 , M_2 : one end of the coils of each of these magnets is soldered to the insulated ring I_2 ; the two other ends are soldered to I_1 and I_3 respectively. The binding-screws U_1 , U_2 , U_3 are connected electrically by means of springs with these rings; U_2 is further connected with the pole N of the battery, while from U_1 and U_3 wires run to E_2 and E_1 respectively. The disposition of the wires is shown in fig. 5: it is there evident that if the mercury in the manometer touches the rod E_1 , M_2 will become magnetic; the loose pulley P_2 (fig. 3) will then tend to revolve with it, and the tap will take up the required position ab ; while if the mercury touches E_2 , M_1 will become magnetic, P_1 will revolve, and the tap will assume the position $a'b'$, in which communication is made with the air-pump. By this arrangement, therefore, the pressure in the manometer, and in whatever apparatus is connected with it, is caused to rise and fall within very small limits; with care these limits may be made to differ only about 0.25 millim.; and thus a practically constant pressure is attained.

The current required to work the magnets is no more than is furnished by a small Smee's cell; the magnets may be made to revolve with a small turbine; the air-pump may be replaced by a Bunsen water-pump; an air-pump, however, is the only convenient apparatus for *compressing* air.

It may be objected that the arrangement above described requires motive power, which is not always at hand in a laboratory. To meet this objection I endeavoured to construct a double valve to be moved to and fro by two stationary magnets; but I found that magnets of ordinary size were not powerful enough for the work, as the valve, to be of any use, must fit perfectly air-tight, even when subjected to very high pressures. I have not, however, given much attention to this point, an engine which I use for many other purposes being obviously the best source of the necessary power.

In fig. 6 is given a section of the steam- or vapour-bath which I employ for the comparison of thermometers and for the measurement of the expansion of liquids in dilatometers; it is also suitable for the direct comparison of thermometers with a standard air-thermometer. The bath is made of brass, and consists merely of a boiler B surmounted by a double tube D , D similar to those first used by Rudberg and Regnault for

the upper fixed point of thermometers; the vapour, after traversing this, passes into the U-shaped condenser C, from which the condensed liquid runs back into B down the tube E. The end H of this condenser is connected with the lateral tube C of the manometer by means of a very small lead pipe; and thus the liquid can be made to boil under any required pressure. The thermometers or other instruments are placed in the small tubes T T, which are filled with petroleum of high boiling-point. Besides water, the best liquids for generating the vapour appear to be carbon disulphide for low, and purified paraffin oil for high temperatures; the latter substance I have not hitherto used, as I have had no occasion to make observations at such temperatures. I have, however, made experiments with it in a smaller apparatus of similar form, and found that no variation of temperature took place.

The temperature obtained from these three liquids may be varied from 25° to 300° without unduly increasing the pressure.

If the temperature in the double tube D D be observed with a thermometer of which each degree is 5 millims. long, no variation can be detected, even with an extremely rigid cathetometer, provided, of course, that the barometric pressure does not alter; the variation due to this cause might, if necessary, be removed by making the cap of the tube F of the manometer fit air-tight. When great accuracy is necessary, it is not advisable to decrease the pressure below 100 millims.

In order to see whether the whole length of D D is at the same temperature, small oblique tubes similar to T were inserted, one at the top, the other at the bottom; a thermometer placed first in one and then in the other of these, gave exactly the same reading in both.

To sum up. First, with the above apparatus, viz. a manometer communicating by means of a specially constructed tap worked by a double electromagnetic clutch with a constantly working air-pump, any given pressure may be maintained for an indefinite period without varying more than 0.25 millim.; secondly, if this constant pressure be applied to a suitable vapour-bath, any given temperature between 25° and 300° may be maintained absolutely constant as long as no chemical change occurs in the liquid whence the vapour is derived.

LXV. *Considerations on the two Memoirs of Sir B. C. Brodie on the Calculus of Chemical Operations.* By M. A. NAQUET*.

IT is not our intention here to express a complete judgment upon the work of Sir B. C. Brodie. This work comprises a mathematical part, of which we are not competent to judge, and a chemical part, upon which we have the right of pronouncing. It is with the latter alone that we mean to occupy ourselves here.

We may begin by saying that whatever may be the final judgment pronounced upon the work of Sir B. C. Brodie, that work appears to us remarkable ; and it is that which has induced us to make it known to the French public.

The application of Algebra to the experimental sciences, the substitution of "theories," based upon facts and demonstrated laws, for "systems" which only rest upon metaphysical hypotheses, is the end towards which science ought to tend ; and if systems are necessary for the arrangement of phenomena, and for the discovery of new phenomena in those points where the progress of science has not yet allowed them to be replaced by theories, this substitution ought, nevertheless, to be effected as soon as practicable.

We will say at once that Sir B. Brodie is unjust in denying the discoveries which are due to the atomic notation. It is by means of this notation and the probabilities deduced by it that a number of syntheses have been effected—such, for example, as the synthesis of the phenols, those of the acids of the salicylic series, of secondary and tertiary ammonia compounds, etc. etc. How could it, in fact, be otherwise ? As M. Dumas says, in his 'Leçons de philosophie chimique,' a hypothesis created for the explanation of twenty phenomena, to which it is adequate, is necessarily applicable to ten, twenty, thirty other unknown phenomena on the track of which it places the observer. But, while fully recognizing the superiority of a "theory" over a system, we refuse to abandon our system unless the theory be complete enough to render all the services which the system has rendered. It does not appear to us that at present the notation of Sir B. Brodie has gone so far as to be able completely to replace the existing notation ; but I do not consider these reasons sufficient to condemn it.

When Gerhardt modified the notation in use before his

* M. A. Naquet has translated into French the two memoirs referred to, on which translations the critical observations of M. A. Naquet are based. These have been translated into English under my supervision.—B. C. B. (*Moniteur Scientifique du Docteur Quesneville*, Nov. 1878, March and April 1879.)

time, this notation did not assume at first the form it has since acquired: it has been perfected, modified, almost transformed; it is, however, from Gerhardt that it dates, and to him the honour of it is due. Similarly Sir B. C. Brodie makes a bold attempt on a new path which may lead to great results; and we must beware of rejecting what he brings on the ground that it is incomplete. The mathematical analysis proposed by him is as yet but a germ; worked out and developed, it may become an organism.

This said, and all reservations made as much in favour of as against the new method, we may say at once that the existing atomic notation may be divided into two parts:—that which is entirely hypothetical and metaphysical, and which explains phenomena by the grouping of atoms; and that which, in spite of the words *atom* and *molecule* which offend Sir B. C. Brodie, is not more hypothetical than the notation of Sir B. C. Brodie himself. The whole is, in fact, a matter of definition. How do we define a molecule? It is the smallest portion of matter at which we can arrive by physical division, and of which the weight is equal to two volumes of the vapour considered in relation to the weight of one volume of hydrogen, both at the normal conditions of temperature and pressure. How do we define an atom? It is the smallest quantity of a given portion of matter attainable by chemical division, which is not subdivided in any operation, and which is always transported integrally from one combination to another. A molecule, then, setting aside all metaphysical senses, is the weight of two volumes of a gas or a vapour; an atom is the weight of two, of one, or of half a volume of a gas or a vapour, according as, in chemical reactions, the molecule is transported intact or is subdivided into smaller weights.

Now what does a "simple weight" represent for Sir B. C. Brodie? It is a weight which in being transported from one combination to another is not "distributed." What does a compound weight represent to him? It is a weight which in the course of chemical operations is divided, is "distributed." Finally, what does he term the "unit of ponderable matter"? The weight of one volume, say of 1000 cub. centims., of a gas or of a given vapour.

Let us suppose that Sir B. C. Brodie had accepted his hypothesis α^2 instead of stopping at the hypothesis α . All his units would then become equal to our molecules; those of his "units" which were not "distributed" would have been identical with those of our molecules (mercury, for instance) which we consider as composed of a single atom. Those of his "units" which were "distributed" would have been

identical with those of our molecules which we consider as composed of several atoms. It is, in fact, absolutely identical to take $H=1$ and to bring all the molecules to two volumes, or to bring all the molecules to one volume and to take $H=\frac{1}{2}$. The numbers obtained would be the same in the two cases.

Now, if Sir B. C. Brodie has been induced, by the serious considerations given in his memoir, to prefer the hypothesis α to the hypothesis α^2 , he does not, however, consider the hypothesis α^2 as more metaphysical, less scientific than the other; and if it had not been his object to explain the "law of even numbers," he would have adopted it*.

With the hypothesis α^2 , there would have been nothing different between our notation and that of Sir B. C. Brodie, nothing except the substitution of the words "units," "distributed weights," "simple weights," for the words "molecules," "molecules composed of several atoms," and "atoms." Now, when it is well understood that no importance, no metaphysical signification, is attached to the words "molecules" and "atoms," but that the expressions are simply taken as indicative of facts previously announced, the question of words signifies nothing, and it matters little whether the same ideas are expressed by "molecules" and "atoms," or by "units," "simple or undistributed weights," and "compound or distributed weights"†. It matters no more than it matters whether a given thought be expressed in one language or in another, provided that the expression be clear and unequivocal.

Let us go farther. Sir B. C. Brodie recognizes that certain "weights" which are regarded absolutely (that is, when the whole of the chemical system is considered) as "compound or distributed," cannot in a limited system of operations be distributed, and act like "simple weights." These are "simple relative weights." Now, when we give the name of "com-

* The fact is that; but for the law of even numbers, the system α could not be constructed at all.—B. C. B.

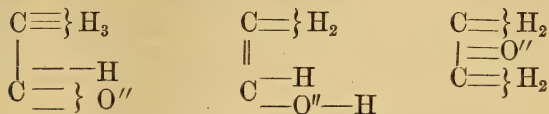
† All this is perfectly true; but it is eminently undesirable to express different ideas by the same term, which leads to confusion. The ideas expressed by the terms "simple weights" and "undistributed weights," are not the ideas expressed by the terms "atoms" and "molecules," but rather those ideas divested of what M. Naquet terms their "metaphysical signification," which are not the same thing. Indeed the advocates of the atomic basis of Chemistry would not, I imagine, be very well pleased with M. Naquet's description of an atom as "a metaphysical entity."

The identity of system α^2 with our present system is limited to the identity of the notation by which the units of matter are expressed. The Algebraical method of working with these symbols as developed in Part II., which is an essential feature of this Calculus and peculiar to it, is not found in our present system.—B. C. B.

pound radical" to the portion of matter represented by C_2H_4 or C_2H_5 , for instance, we do not express any other idea. The compound radical represents to us a portion of matter which in the series of phenomena under consideration is transported integrally from one combination into another, although it may, in other circumstances, be decomposed into simpler elements. "Compound radical," or "simple relative weight," are therefore one and the same thing. Here, again, is a matter of words.

We may add that the substitution of hypothesis α for hypothesis α^2 is not fundamental. If, supposing that Sir B. C. Brodie had not written his two memoirs, it had been shown that the bodies, so called, of uneven atomicity contain hydrogen, and correspond to the general formula HR_2 or H_2R_4 , immediately our existing notation would be transformed, except for the use of Greek letters, into that of Sir B. C. Brodie, without any alteration in the general system, unless it were the important discovery of the compound nature of a series of bodies hitherto considered as elementary.

To set against this there is in the existing notation a series of hypothetical considerations for the expression of *fine* isomers, which Sir B. C. Brodie rejects. Thus, when we express the isomeric relations of aldehyde, acetylenic alcohol, and oxide of ethylene by the formulæ



we make a hypothesis on the mode of grouping these atoms considered as metaphysical entities.

It is the same when we try to account for the differences existing between isomers such as salicylic, oxybenzoic, and paraoxybenzoic acids, and explain them by the place occupied in relation to each other by the groups OH and CO_2H and the benzol-group C_6H_6 .

We will return presently to this hypothetical part of our notation. Let us pause for a moment on the part common to the existing notation and the notation of Sir B. C. Brodie, and see if, in fact, the hypothesis α , setting aside the difficulty caused by the necessity of considering as compounds a mass of simple weights, offers the advantages over the hypothesis α^2 which Sir B. C. Brodie asserts. According to him, a hypothesis is acceptable when it accounts for all the known facts. Of two hypotheses which equally account for all the facts, that

one ought to be preferred which is the more limitative. In other words, if a hypothesis A accounts for all the facts and does not suggest any improbable phenomenon, while another hypothesis A_1 accounting equally for all the known facts suggests a considerable number of unknown and improbable facts, it is hypothesis A which we ought to select.

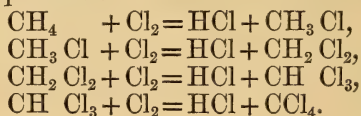
We fully accept these principles; but let us see their conclusions. The hypothesis α^2 , Sir B. C. Brodie says, permits the regular explanation, by means of integral positive factors, of the symbols of all the known facts. But it admits besides of the similar construction of symbols of an equal number of substances which the law of even numbers rejects as impossible to realize. The hypothesis α , on the contrary, while permitting the expression by symbols, by means of integral positive factors, of known facts, excludes the possibility of representing in the same way, by means of symbolic expressions, the substances which do not obey the law of even numbers. The hypothesis α is therefore superior to the hypothesis α^2 .

Such is the argument of Sir B. C. Brodie; and we should consider it irreproachable if hypothesis α were not, though more limitative on one side, more extensive on the other, and if, from this point of view, what is gained in one sense were not lost in an opposite sense.

But, first, the hypothesis α compels us to admit that chlorine, bromine, iodine, nitrogen, phosphorus, arsenic, antimony, bismuth, potassium, sodium, etc., are compound bodies corresponding to the general formula HR_2 or H_2R_4 . This is a serious obstacle against accepting this hypothesis; for though there may be nothing impossible in this supposition, neither is there anything demonstrated: we anticipate experience, and thus we enter upon a path which threatens to lead us far.

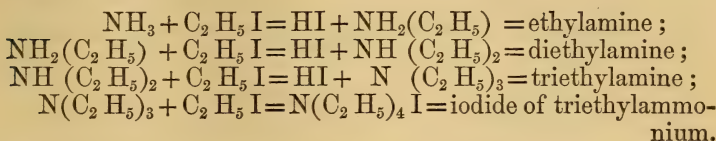
This is not all. The existing theory, which considers chlorine, bromine, iodine, nitrogen, etc. as elementary bodies, leads us to consider as products of "substitution" the compounds resulting from the action of chlorine, bromine, or iodine on organic hydrogenized bodies, as well as the compound ammonias produced by the action of simple ethers on ammonia. The number of these products is thus limited.

Let us take, for example, the action of chlorine on marsh-gas, CH_4 . Theory indicates that this action ought to produce four bodies, and four bodies only, as indicated in the following equations:—



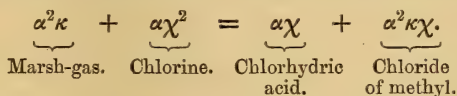
And, in fact, the action of chlorine on marsh-gas produces four products of substitution—chloride of methyl CH_3Cl ; chloruretted chloride of methyl, CH_2Cl_2 ; chloroform, CHCl_3 ; perchloride of carbon, CCl_4 —and four only.

Similarly, ammonia having as its formula NH_3 , and chloride of ammonium NH_4Cl , we ought to be able to substitute three alcoholic radicals for the hydrogen of ammonia, and four of these radicals for the hydrogen of ammonium contained in the ammonia chloride, whence result three compound free ammonias and three only, four compound ammoniums in the state of combination and four only, as indicated by the following equations:—



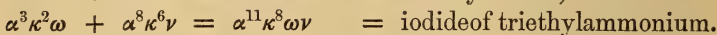
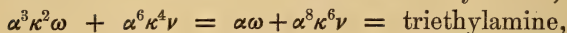
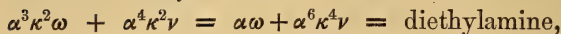
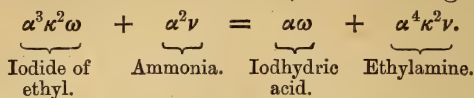
And this is, in fact, what occurs: each compound monatomic alcohol-radical produces three compound free ammonias and one quaternary ammonium, and these four ammonia derivatives only. Finally, from the relations which exist between the derivatives of the chlorine substitution of hydrocarbons and alcohols, the latter also are considered as products of substitution resulting from the exchange of H, Cl, Br, or I for the group OH. From the relations which connect alcohols to aldehydes, aldehydes to acids, acids to amides, the number of alcohols, acids, aldehydes, and amides is limited, like that of the chlorine derivatives, by the number of chlorine derivatives contained in each hydrocarbon.

It is not the same with Sir B. C. Brodie's notation. Marsh-gas, for example, being $\alpha^2\kappa$ ($=\text{CH}_2$, $\text{C}=6$), and chlorine being $\alpha\chi^2$, the reaction of chlorine on the hydrocarbon becomes

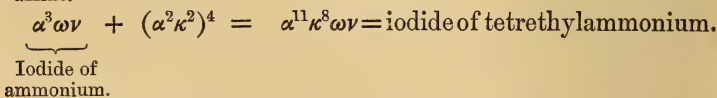
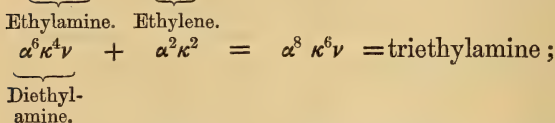
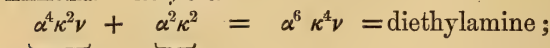
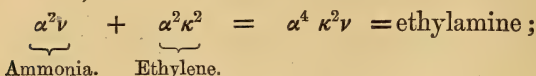


That is to say, the chloride of methyl, which in our notation corresponds to the formula CH_3Cl , and represents a product of substitution of chlorine for hydrogen, in Sir B. C. Brodie's notation represents a simple addition of the prime factors of chlorine χ to marsh-gas. Similarly, methylic alcohol, CH_3OH , which in our notation is a product of substitution, becomes in the new notation $\alpha^2\kappa\xi$ —that is to say, a simple product of addition.

It is the same for the compound ammonias. The equations by which we have represented the formation of these bodies take, in Sir B. C. Brodie's notation, the following form:—

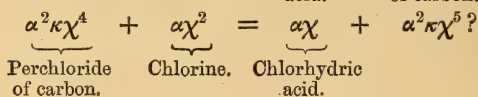
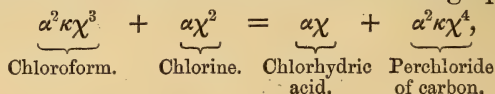


That is to say, the ammonias and the compound ethylic salts of ammonium result simply from the addition of ethylene, $\alpha^2 \kappa^2$ to ammonia, or to the salts of ammonium. We have, in fact,



Now, since Sir B. C. Brodie rejects every atomic speculation which would necessarily place a limit to these different additions, his theory shows for each hydrocarbon an indefinite number of chlorine, bromine, or iodine derivatives, and an equally indefinite number of alcohols, aldehydes, and acids; it shows also an indefinite number of compound ammonias for each alcohol, of amides for each acid, &c. &c.

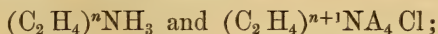
We can, in fact, according to the new system of notation, perform upon the symbol of perchloride of carbon, $\alpha^2 \kappa \chi^4$, the same operation that we can perform upon marsh-gas, perchloride of methyl, chloruretted chloride of methyl, and chloroform themselves. Thence we obtain the following equations:—



It is absolutely impossible to express this last compound in our existing notation.

Similarly, tetratomic alcohol, which would have for its formula $C(OH)_4$, an alcohol which is unknown, but of which the derivatives are known, ought to be written, according to Sir B. C. Brodie, $\alpha^2\kappa\xi^4$. But Sir B. C. Brodie's theory shows equally an alcohol $\alpha^2\kappa\xi^5$, which our existing notation rejects as impossible.

Finally, besides the triethylamine, $(\alpha^2\kappa^2)^3\alpha^2\nu$, and the iodide of tetrethyl-ammonium $(\alpha^2\kappa^2)^4(\alpha^2\nu)(\alpha\omega)$, the notation of Sir B. C. Brodie shows the compound ammonias $(\alpha^2\kappa^2)^n\alpha^2\nu$ and the compound iodides of ammonium, $(\alpha^2\kappa^2)^{n+1}(\alpha^2\nu)(\alpha\omega)$. These last bodies, unlike the preceding, might be written in our existing notation, which would permit the expression of these compounds by the formulæ



but our notation allows us also to consider them as compounds of substitution, instead of considering them as compounds of addition, which limits their number.

Now, up to the present time there has never been obtained a number of chlorine, bromine, or iodine derivatives of a hydrocarbon greater than the number of the atoms of hydrogen which this hydrocarbon contains. There has never been obtained a number of alcohols greater than that of the chlorine, bromine, or iodine derivatives. Finally, there has never been obtained for each monatomic alcohol a number of compound ammonias greater than three, and a number of compound salts of ammonium greater than four.

On the other hand, the law of even numbers, which Sir B. C. Brodie has so much at heart, is far from being demonstrated, since the exception of the oxides of nitrogen cannot be eliminated unless we admit the dissociation of our element nitrogen. But even if the law of even numbers were absolutely demonstrated, it would still not prove that a body which cannot exist in a free state may not exist in a state of combination. And, finally, if this explanation itself be inadmissible, since the notation α of Sir B. C. Brodie shows an innumerable multitude of improbable bodies, and he does not point out to us any rule for eliminating them, which is contrary to that which actually occurs in the existing notation (and to that which occurs in the notation of Sir B. C. Brodie), for the bodies which do not satisfy the law of even numbers, the hypothesis α appears to us in all points inferior in the existing state of science to the hypothesis α^2 .

Now the hypothesis α^2 is nothing else than our existing

notation without the speculations explanatory of the *fine* isomers mentioned above. Are these speculations, which could be perfectly well rejected even with our existing notation, out of date and already useless? I do not think so. They may become useless one day. We shall perhaps find in thermochemistry the means of explaining these *fine* isomers, and applying to them mathematical expressions. But until then these speculations appear to us useful, because they arrange facts which would otherwise remain without arrangement; and they correspond well enough to the actual phenomena to allow us to determine, notably in the aromatic series, the number of these *fine* isomers possible for each term of the series.

Even if we do not take into consideration the extreme difficulty that there is in replacing one notation by another, and the momentary confusion introduced by it into science, a confusion which ought not to be permitted unless the change offers an undeniable advantage, even if we do not take into consideration this difficulty, we should oppose the immediate adoption of Sir B. C. Brodie's notation, because it seems to us to open the door to a crowd of suppositions more considerable than those it aims at eliminating, and because, from the point of view of the arrangement and anticipation of facts, it does not provide us with the means of dispensing with those atomic speculations which, without being intimately connected with the existing notation, may nevertheless be joined to it, and cannot in any case be adapted to the notation of Sir B. C. Brodie.

Are we then to conclude that Sir B. C. Brodie's work has no value? Far from us indeed be such a thought. If we had been of this opinion, we should not have taken the trouble to translate it. It opens out a new method which, enlarged and perfected, will permit the application of Algebra to Chemistry, and the substitution for our "system of chemistry" of a true "theory of chemical events." Even if this result be not produced, if the imperfections with which we charge the new method of notation continue to exist, if there only remain of the notation of Sir B. C. Brodie his hypothesis α , including that of the compound constitution of chlorine, nitrogen, potassium, and their compounds, the work would still be useful.

As long as we have to do with a "system," a hypothesis with no other object than that of arranging known facts and discovering new facts, two different hypotheses may legitimately be employed, provided that they answer to the requisite conditions. It is thus that the hypotheses of emission and undulations have long been tacitly accepted in Optics. It

might happen in this case also that two different hypotheses, both including the whole of the known phenomena, should lead to different deductions, and present different consequences which should guide the operator into two distinct paths. When things occur thus, it is useful to accept the two hypotheses at once, and since no objective reality is attributed to them until experience has decided (when experience can decide), both are undeniably useful. Sir B. C. Brodie's system, by showing the possible decomposition of the whole of one class of our elements, and by indicating one of the probable cases in which these elements are decomposed (the cases of binoxide and tetroxide of nitrogen), renders a real service to chemical science, and deserves to be known and studied. It will deserve this still more, if it be found that in working it out and perfecting it its defective sides are caused to disappear and its completion effected. It will help to place Chemistry on a solid foundation; and such an attempt has a right to the sympathetic attention of the whole scientific world.

Postscriptum. Since these lines were written some curious observations have allowed scientific men to consider as possible, as even probable, the production of free hydrogen by the action of an extremely elevated temperature on the greater number of our elementary bodies. It is useless to demonstrate—it demonstrates itself—the value which this discovery, if it is confirmed, would give to Sir B. C. Brodie's hypothesis.

Note on an Objection made by M. Naquet in his preceding "Observations." By Sir B. C. BRODIE, F.R.S.*

M. Naquet has had the kindness to forward to me the proofs of his "Considerations" on my Memoirs. It is not my wish to comment here on his remarks; but there is one point in reference to which the remarks of M. Naquet are founded on a pure misapprehension of the state of the case. This point is of fundamental importance; and I will give a few words of explanation in regard to it. Indeed, if the difficulties which he has created really existed, the atomic method would have in some respects a great advantage over the method of this Calculus. I will explain the matter as briefly as possible. The objection of M. Naquet is this. We may, according to the atomic method, operating by way of substitution of atom for atom, foresee in certain cases the number of practicable substitutions. Thus, marsh-gas (CH_4) containing four atoms of hydrogen, we may substitute in it chlorine for hydrogen

* From the *Moniteur Scientifique* of Dr. Quesneville, April 1879.

four times and no more. Similarly in ammonia (NH_3) three analogous substitutions are possible. Whence we have, for example, the three ammonia bases, methylamine, dimethylamine, trimethylamine. Again, in chloride of ammonium (NH_4Cl) we may have four such substitutions; and extending this principle, we are able to anticipate in numerous cases the precise number of similar derivatives. All this, it is asserted, is the work of the atomic method. Now the theory before us, says M. Naquet, does nothing of the kind, and, indeed, is incapable of doing it. We have $\alpha^2\kappa\chi^3$ as the symbol of chloroform, and $\alpha^2\kappa\chi^4$ as the tetrachloride of carbon. All that is here done to form the chlorine derivatives of marsh-gas is to add the weight χ over and over again to the weight $\alpha^2\kappa$. Go on in the same direction, the next step brings you to $\alpha^2\kappa\chi^5$, an utterly impossible, or at least an eminently improbable, entity, which yet is not excluded from the system.

The reply to this is that M. Naquet does not give a correct account of the process by which these derivatives are constructed. We cannot make $\alpha^2\kappa\chi^5$ by the same process as that by which $\alpha^2\kappa\chi^4$ is manufactured. After $\alpha^2\kappa\chi^4$ is placed an insurmountable barrier to progress in this direction which he does not see. He can go so far, but no further.

M. Naquet's criticism is based on a distinction for which he certainly is not responsible, as it is made in every chemical treatise, which is necessitated by the material mode of treatment of the atomic theory, but which, in this Calculus, does not exist, namely the distinction between Addition and Substitution of atoms. For us the two processes are merged in one. (Part II. Section III. (9).)

Let us consider the equation which expresses the relation between the chloride of iodine and its constituents,

$$\alpha\chi^2 + \alpha\omega^2 = 2\alpha\chi\omega,$$

or

$$\alpha\chi^2 + \alpha\omega^2 - 2\alpha\chi\omega = 0.$$

This equation vanishes when $\chi = \omega$. It may be written thus

$$\alpha(\chi - \omega)(\chi - \omega) = 0.$$

In this event α is constant, and it occurs in two ways by the substitution of ω for χ , which substitution is expressed by the symbol $(\chi - \omega)$, and we have:—

Symbol of the unit of chlorine.....	$\alpha\chi\chi$,
„ „ chloride of iodine...	$\alpha\omega\chi$,
„ „ iodine	$\alpha\omega\omega$,

in which symbols the relation of substitution connecting these units is apparent.

Now taking the equation

$$\alpha + \alpha\chi^2 = \alpha^2\chi,$$

we have similarly

$$\alpha(\chi-1)(\chi-1)=0,$$

an event in which α is constant, and which occurs in two ways by the substitution of 1 for χ —that is, by the operation $(\chi-1)$. Reasoning as before, we have:—

Symbol of the unit of chlorine	$\alpha\chi\chi$,
„ „ hydrochloric acid ..	$\alpha\chi 1$,
„ „ hydrogen.....	$\alpha 11$.

Now the symbol 1 which appears in the symbols of hydrochloric acid and of hydrogen is not the symbol of any real weight, but is the symbol of an empty unit of space serving to mark the place where matter is not, but has been, and may be again. The explicit introduction of this symbol into the symbol of the unit of hydrogen limits the number of the operations χ (or of any operation which may be substituted for χ) which may be performed upon α , the unit of hydrogen, to two. Thus the operations ω or β (which may be substituted for χ) may be performed once and twice upon the unit of hydrogen, but no more. Hence we have:—

Symbol of the unit of hydrogen.....	$\alpha 11$,
„ „ hydriodic acid.....	$\alpha\omega 1$,
„ „ iodine	$\alpha\omega\omega$;

but we have no means of making $\alpha\omega^3$.

Now in the symbol $\alpha^2\kappa$, the symbol of the two combined units of hydrogen, which (regarded purely as the symbol of the weight of the combined hydrogen) is α^2 , becomes, by the explicit introduction of the symbol 1, $\alpha^2(1, 1, 1, 1)$, and the symbol of marsh-gas is $\alpha^2\kappa(1, 1, 1, 1)$. In this case four successive substitutions of χ for 1 are possible, by which the following units are constructed:—

$$\begin{aligned} &\alpha^2\kappa(\chi, 1, 1, 1), \\ &\alpha^2\kappa(\chi, \chi, 1, 1), \\ &\alpha^2\kappa(\chi, \chi, \chi, 1), \\ &\alpha^2\kappa(\chi, \chi, \chi, \chi), \end{aligned}$$

which are the symbols of the four chlorine derivatives of marsh-gas. But we have no means of constructing the unit $\alpha^2\kappa\chi^5$. (Part II. Section III. (12).)

Again, the unit of nitrogen $\alpha\nu^2$ is derived from the unit of hydrogen by the substitution of ν for 1 in that unit; and we have:—

$$\begin{aligned} &\alpha(1, 1) \text{ hydrogen,} \\ &\alpha(\nu, 1) \text{ (unknown),} \\ &\alpha(\nu, \nu) \text{ nitrogen.} \end{aligned}$$

Effecting this substitution not in one but in two combined units of hydrogen, we have:—

Symbol of two combined units of hydrogen $\alpha^2(1, 1, 1, 1)$,

Symbol of the unit of ammonia $\alpha^2(\nu, 1, 1, 1)$.

It hence appears that we can effect in the unit of ammonia three substitutions of χ (or of any value of χ) for 1, but no more. Now $\alpha\kappa$ is the symbol of such a value of χ , a fact ascertained by repeated experiments; and the symbols of these three derivatives are:—

$$\alpha^2\nu(\alpha\kappa, 1, 1),$$

$$\alpha^2\nu(\alpha\kappa, \alpha\kappa, 1),$$

$$\alpha^2\nu(\alpha\kappa, \alpha\kappa, \alpha\kappa);$$

hence the equation

$$3\alpha^2\kappa\omega + \alpha^2\nu = 3\alpha\omega + \alpha^5\kappa^3\nu$$

expresses not only a result, but the final result of the action of iodide of methyl on ammonia.

In the case of the chloride of ammonium, $\alpha^3\chi\nu$, putting

$$\alpha^3(1, 1, 1, 1, 1, 1)$$

as the symbol of three combined units of hydrogen, we have, effecting the substitution of χ for 1 and of ν for 1,

$$\alpha^3\chi\nu = \alpha^3(\chi, \nu, 1, 1, 1, 1),$$

in which four substitutions, and no more, of $\alpha\kappa$ for 1 may be effected; whence we have

$$\alpha^3(\chi, \nu, \alpha\kappa, \alpha\kappa, \alpha\kappa, \alpha\kappa)$$

as the symbol of the unit of the chloride of tetra-methyl-ammonium.

From the point of view of general algebra, $\alpha 1^2 = \alpha$, $\alpha^2\nu 1^3 = \alpha^2\nu$, $\alpha^2\kappa 1^4 = \alpha^2\kappa$, $\alpha^3\chi\nu 1^4 = \alpha^3\chi\nu$; we are therefore at liberty either to suppress the symbol 1 or to exhibit it according to our convenience. We may compare the symbol $\alpha^2\nu(1, 1, 1)$ to an open fan, $\alpha^2\nu$ to the same fan folded up. I have said enough to clear up the difficulty of M. Naquet; and this is not the place to further pursue the subject.

With reference to the problem of the expression of "Isomers" on the principles of the Calculus, I will venture to ask M. Naquet and others to suspend their judgment for a time*.

It is a significant fact that a very large proportion of the class of elements which I have termed composite elements have not been found in the sun. In reply to inquiries on my part, Mr. W. Huggins writes to me thus:—

* The following passage and also the footnotes have been added since the publication in the *Moniteur Scientifique*.

"So far as I know, nitrogen, phosphorus, arsenic, antimony, boron, chlorine, iodine, bromine, have *not* been found in the sun. In one paper Lockyer *suspects* iodine. Dr. Miller and I found coincidence of three lines of antimony with three lines in aldebaran. Though this observation would show considerable *probability* of antimony in star, I do not think the spectroscope (two dense prisms of flint glass) was sufficiently powerful to make its existence there certain. In the case of nitrogen, no coincidence was observed in any of the stars. In my paper in the Transactions of the Royal Society on Spectra of Nebulæ, I show coincidence of principal line with the *strong* line in spectrum of nitrogen. Now this line of nitrogen is a double one; and I was not at first able to be certain if the line in the nebula was similarly double. Subsequently with the powerful spectroscope I used for the motions of stars, I was able to make a *certain* determination of this point (Proceedings R. S. 1872, p. 385). I found the line in the nebula *single* and *coincident* with the middle of the less refrangible of the components of the double line

Nitrogen.	Red.
Nebula.	

I say "middle" because line in the nebula is narrower and more defined than either of the two lines forming the double line. I made experiments to see if under any conditions of pressure and temperature the more refrangible of the two lines fades out, so as to leave only the one with which the line in the nebula is coincident. I did not succeed. So the matter stands:—Is nitrogen compound? Are there any conditions under which the one line only appears? Has the line in the nebula no real connexion with nitrogen further than being sensibly of the same refrangibility?"

Now we must either consider that the matter of these elements so abundant on the earth does not exist in the sun or stars (which is hardly probable), or that they have passed into forms of combination in which they cannot be recognized by the spectroscope (which is also hardly admissible at that elevated temperature), or that they have been decomposed.

Second Note of M. A. Naquet.

Sir B. C. Brodie replies victoriously to the principal objection which we brought against him, and begs us to suspend our judgment on the second objection. We will therefore await the new memoir which his last sentence allows us to expect.

There remain two objections to oppose to Sir B. C. Brodie; but they are of less importance. The first is that Sir B. C. Brodie admits, without any experimental proof, the compound nature of certain bodies considered simple, like chlorine and potassium, while others, like mercury, remain simple—although it appears, according to the law of Dulong and Petit, that all our elements ought to be compound, or none of them ought to be compound, at least of those to which the law of Dulong and Petit is applicable. We repeat, however, that though Lockyer's experiences do not conclusively establish the compound nature of our elements, this would be one step towards the verification of Sir B. C. Brodie's hypothesis.

The second objection is that the law of even numbers, which serves as the basis of the new notation, can be thoroughly established in the series of carbon combinations, but it cannot be so completely established in the other series. It would perhaps be simpler to admit that this law is not universal than to admit that chlorine and nitrogen are compound bodies. In any case we cannot do better than repeat, in conclusion, that which we have already said. All hypotheses are but mental artifices to guide us to the discovery of truth; and since a new hypothesis opens new horizons to the investigator, this hypothesis may be fruitful, and ought to be accepted, either by replacing an old hypothesis which has become sterile, or by concurrence with it. It is for this reason that we have translated Sir B. C. Brodie's memoirs; it is for this reason that we do not understand indifference towards his work, and that we should understand such indifference still less now that he has done away with one of the two principal difficulties which we thought we had found in his system, and that he promises before long to do away with the second.

LXVI. *On a new Application of the Potential Energy of Liquid Surfaces.* By Professor G. VAN DER MENSBRUGGHE*.

IN a recent memoir† I sought to confirm my theory of the variations of potential energy of liquid surfaces by presenting a series of proofs drawn from the observation of liquid films either with two free faces or spread upon another liquid. I had hardly finished the writing of that memoir when my attention was drawn to the remarkable phenomena presented

* Translated from a separate impression communicated by the Author, from the *Bulletins de l'Académie Royale de Belgique*, 2^e série, t. xlv. no. 11, 1878.

† "Études sur les variations d'énergie potentielle des surfaces liquides," *Mém. de l'Acad. royale de Belgique*, 1878, t. xliii.

by the plane or curved sheets of liquid first described by Savart*, and afterwards studied by Hagen†, Tyndall‡, Magnus§, and Boussinesq||. I quickly recognized some very interesting verifications of my theory in the curious peculiarities manifested by those sheets, of which no satisfactory explanation has, to my knowledge, been yet furnished. In order to be able to continue without hurry the experimental control of my theoretic deductions, I will briefly explain how the principles of thermodynamics account for the effects in question.

2. It is known from Savart's observations that, if two veins of water with equal circular sections are impelled with equal and opposite velocities, and meet so that their axes coincide, there is formed, for all pressures sufficiently strong, a plane circular sheet bounded by a rough, agitated, and sonorous zone when the pressure exceeds a certain amount, but which for a less charge becomes perfectly smooth and even throughout its extent. From this it follows that, under favourable conditions, not merely the *vis viva* of the water after the impact of the two veins, but also the effects of gravity are almost completely nullified after a relatively very short course. What can be the cause that destroys so considerable an energy of motion? and by what is the latter replaced?

3. Since 1849 M. Hagen has invoked, as being that cause, the superficial tension of the liquid, which, having constantly to be overcome, gives rise to a retarding force.

Doubtless the theory of M. Hagen is very ingenious, especially for the time when it was sent forth; but, besides that it does not clearly show the force which is substituted for the *vis viva* in proportion as this is destroyed, it does not make intelligible the various effects ascertained by Savart, except by means of an indispensable complement or, rather, a rectification. In the actual case the principle of the conservation of energy is verified, owing to the circumstance that the energy of motion of the two veins is almost entirely replaced by the potential energy of the superficial layers of the two faces of

* "Mémoire sur la choc d'une veine liquide lancée contre un plan circulaire," *Ann. de Chim. et de Phys. de Paris*, 1833, t. liv. p. 55; "Mémoire sur le choc de deux veines animées de mouvements directement opposés," *ibid.* t. lv. p. 257.

† "Ueber die Scheiben, welche sich beim Zusammenstossen von zwei Wasserstrahlen bilden, und über die Auflösung einzelner Wasserstrahlen in Tropfen," *Pogg. Ann.* 1849, vol. lxxvii. p. 451.

‡ On some Phenomena connected with the Motion of Liquids," *Phil. Mag.* 1854, ser. 4, vol. viii. p. 74.

§ "Hydraulische Untersuchungen," *Pogg. Ann.* 1855, vol. xcvi. p. 1.

|| Théorie des expériences de Savart sur la forme que prend une veine liquide après s'être choquée contre un plan circulaire," *Comptes Rendus*, 1869, t. lxxix. p. 45, 128.

the sheet. On the other hand, the principles of thermodynamics require that the kinetic energy shall diminish, not only because the concentric rings widen, but moreover because, precisely on account of the superficial enlargement on the two faces, the tension and with it the retarding force are more and more augmented. This last point follows as a consequence from my deductions already confirmed by a former series of experiments, which are described in my recent memoir.

4. Applying my theory to Savart's plane films, I arrived at the following results:—

(a) The retarding force due to the augmentation of potential energy goes on increasing in proportion as the liquid travelling over the disk removes further from the axis.

(b) As we have $\epsilon = \frac{C}{vr}$ nearly, ϵ being the thickness of the film at any point, C a constant, v the velocity at that point, r the distance from the axis, we conclude that, starting from the axis, ϵ continually diminishes, since r increases more rapidly than v lessens. This gradual diminution of ϵ will cease when vr no longer sensibly changes; but soon the velocity decreases faster than r increases, and from that time the thickness must go on increasing by degrees until it attains a maximum.

(c) When the velocity shall have become sufficiently reduced, ϵ may pass through a series of maxima and minima, so that there will be formed, for sufficiently strong pressures, a series of circular waves joined to one another by cross striæ, which will give rise to the production of minute drops, taking away from the outermost zone its transparence and its regularity of form.

(d) If the velocity of the liquid, after the impact, increases in a certain ratio, the retarding force increases in a greater ratio—which explains the remarkable fact that the liquid disks may become less in diameter when the charge is increased.

These theoretic results are verified in all points by the experiments of the French physicist, and by the measurements furnished by them to M. Hagen.

5. If my theory is susceptible of useful applications in the study of plane liquid sheets, it finds still more curious applications in relation to curved ones. According to Savart, such a sheet is obtained, for instance, by letting a liquid vein, shot through an orifice of 12 millims., under a sufficient pressure, fall upon the centre of a horizontal brass disk fixed at the distance of 20 millims. beneath. After impact the liquid spreads out in all azimuths, and gives rise to a curved film bounded by an indented margin. For a pressure of 2 metres the

sheet is thin, even, and transparent in its central part, and presents towards its contour the aspect of an annular zone or aureola covered with numerous circular striæ connected by radial ones. In proportion as the charge is lessened the diameter of the sheet increases, the aureola becomes more transparent, narrower, and entirely disappears when the pressure at the orifice does not exceed 60 centims. The sheet then attains its maximum diameter (80 centims.), and affects the form of a wide cap, the concavity of which is turned downwards.

To the whole of these phenomena the propositions enunciated at no. 4 are applicable.

In proportion as the charge decreases, the sheet gradually becomes less in diameter, and at the same time curves back upon itself at its lower part, going toward the stem that sustains the disk; at a pressure of about 32 centims. the sheet closes up entirely, assuming the form of a solid of revolution with a perfectly even surface.

The formation of the closed figure is due, as has already been pointed out by M. Plateau, to the effect of capillary pressures in the lower portion of the sheet.

6. We now come to the truly singular transformations observed by Savart.

Directly after the closing of the sheet its dimensions diminish, at first gradually, simultaneously with the charge; when this no longer exceeds 10 centims. the shape of the sheet abruptly changes: the upper part suddenly becomes concave, rising above the plane of the disk; then, after an extremely short time, the former shape reappears; and these instantaneous changes of aspect are periodically repeated seven or eight times, until the sheet entirely vanishes.

Savart, who most carefully studied these abrupt changes, vainly endeavoured to penetrate the cause of them. Since then M. Boussinesq has essayed to give the mathematical theory of the formation of the even and closed sheets; but, like M. Hagen, he regards the capillary constant as remaining the same in every part of the surface. His calculations, too, are not in accordance with experiment, and do not exhibit the cause of the instability of the sheets under certain conditions.

The following are the propositions to which my theory has conducted me, and which, for the most part, I have verified by direct observation:—

(a) To every quantity of energy of motion destroyed corresponds necessarily, as in the case of the plane sheets, an equivalent amount of potential energy, the seat of which is the whole of both faces of the upper portion of the sheet,

limited at the equatorial section (I thus name the section the plane of which passes through the points where the tangent to the generatrix is vertical).

I have, in fact, ascertained that, if the operation is conducted with a constant charge, there is never any sudden rising followed by the formation of a surface concave upwards; no more is there such when the charge, instead of diminishing, goes on increasing; and, finally, there is none when, from any cause whatever, the sheet presents an aperture.

(b) While the gradual development of potential energy in the upper portion of the sheet gives rise to a retarding force, there is developed, on the contrary, in the lower portion an accelerating force, due not merely to gravity, but also to the diminution of the potential energy of the liquid rings, which are incessantly narrowing right to the axis. It is owing to the increase of the velocity of the liquid that the water-threads, after encountering the axis, scatter in little drops. A Savart closed sheet thus presents a striking example of the transformation of kinetic into potential energy, and of potential energy into energy of motion.

(c) If while the sheet is closed the kinetic energy diminishes, either gradually or abruptly, the retarding force arising from the increase of potential energy in the upper part increases, either continuously or suddenly, and then struggles against the accelerating force that animates the lower part of the sheet. For this reason the film is strongly stretched: there is sometimes delineated a projecting ridge; and immediately afterwards the sheet rises, becoming concave upwards; but then the retarding force which dominates the concave portion is directed downward, like gravity and the accelerating force of the lower part—which immediately brings back the sheet to its primitive form, but with smaller dimensions.

While Savart generally operated only with charges decreasing in a continuous manner, I have verified the preceding theoretic consequence by abruptly diminishing the charge 10 centims. I thus saw, after a few seconds, the singular figure, concave upward, formed which so much preoccupied the French physicist.

(d) According to my formula

$$dQ = At d \left(S \frac{dT}{dt} \right),$$

which gives the variation of heat dQ corresponding to an increment dS of the surface S , having the potential energy T , and absolute temperature t , the variation dQ must vanish with

the differential coefficient $\frac{dT}{dt}$; and in that case the potential energy T of the liquid does change; consequently the retarding force in question above is not augmented then, as in the general case; and for this reason the diameter of the sheet must be greater than usual.

This curious result of my theory is fully confirmed by Savart's experiments. With an orifice of 3 millims. the aureoled open sheet had a diameter of 20 centims. when the water was at $1^{\circ}3$ C. and was impelled by the pressure of 4.885 metres; while the sheet under the same pressure, but at the temperature of 4° C., had a diameter of 36 centims.—that is to say, nearly four times the surface. In my opinion, this considerable difference simply proceeds from the potential energy T of the water at its maximum of density having a maximum value, so that $\frac{dT}{dt} = 0$.

(e) If the foregoing conclusion is accurate, in operating with water at 10° C., for example, a cooling must be found to take place in the sheet, while, on the contrary, if the water is at first at, say, $1^{\circ}3$, it must grow warmer in spreading.

(f) Since, as I have proved in a previous investigation, to every thermal variation a difference of electric potential corresponds, the spreading-out of water at its maximum of density into a sheet cannot give rise to a thermoelectric current, provided the temperature in it does not change; on the contrary, the formation of the liquid sheet must be accompanied by a current in one direction if the initial temperature of the liquid be above $4^{\circ}5$, and in the opposite direction if it be below $4^{\circ}5$.

I purpose soon to control this important consequence of my formula. If direct observation verify it, I shall therein find a brilliant confirmation of the theory I am seeking to introduce into science.

LXVII. *On the Detached Colorimeter, and on Colorimetry.*

By EDMUND J. MILLS, D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College, Glasgow*.

THE ordinary detached colorimeter consists of two equal test-tubes mounted on feet. It is a simple, but in many respects an imperfect, instrument. Several of its defects were removed by the Portable Colorimeter†, which has been found to work well in the majority of cases. Room, however, has

* Communicated by the Author.

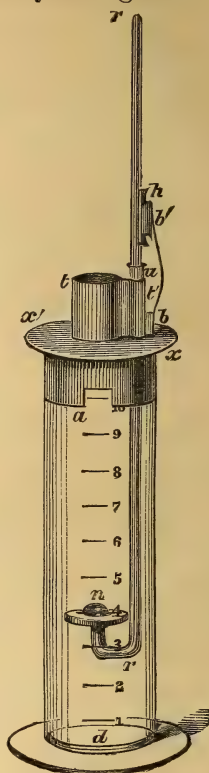
† Proc. Phil. Soc. Glasgow, x. p. 310.

still been left for an apparatus from which leakage at the bottom is impossible, and in which, as in the common detached colorimeter, the liquid comes into contact only with glass.

The new detached colorimeter* is made in two pieces, alike in every respect; one of these is represented in the subjoined figure:—It consists of a stout glass tube having a broad flat foot, and graduated into 100 equal parts; its capacity at the upper part is about 120 cubic centims. On the top of this is a loosely fitting brass cap, prolonged downwards so as to cover and shade the surface of the liquid, thereby preventing the appearance of a dark meniscus. The surface of the liquid is only visible sideways through the little aperture *a*, cut out for that purpose. The cap is perforated centrally; and a short tube *t* rises from the perforation. This tube is soldered laterally to a narrower one *t'*, and this again to a small block *b*, from which rises a spring carrying another small block *b'*. The tube *t'* has, cemented into it, a glass tube *u*, which passes straight downwards, and reappears below the flat surface of the cap, its end amply clearing that surface. This tube is coned outwards at its upper extremity, but is left plain below. Through it there passes, with just sufficient room to move, the rod *rr*, bent below twice at right angles, so as to carry a flat circular “opal” glass disk, to which it is attached by fusion. These disks are turned in the lathe: their surfaces should be polished free from scratches, and their edges show no bevel. The rod is prevented from falling by the easy pressure of the little half-tube *h*, carried by the block *b'*. When the thumb and fore finger are lightly pressed on *x* and *x'*, the rod can be readily moved up and down, and will then stay in any position in which it may have been left. It is convenient to cone outwards the half-tube *h* at both its ends; but only traces of liquid ever reach this spot.

The instrument has two accessories which are of considerable service. These consist (1) of a pair of glass disks, *d*,

* The instrument has been made for me by Messrs. Cetti and Co., Brooke Street, Holborn.



Scale $\frac{1}{3}$.

lying at the bottom of the tube, one having a suitable red, the other a green colour; there is thus obtained a black ground, on which the opal disk is always seen through *t*. An annulus of deeper tint than a given observed colour would otherwise surround the opal disk, and tend to confuse the determination. It is an advantage at times to use other colours, and even to cover the opal disk with a plate of coloured glass. The other accessory is (2) a black hemispherical button *n*. This lies loosely on the opal disk, as shown in the figure. It is used in the estimation of turbidities (*i. e.* precipitates), by lowering it until its point just disappears.

In taking readings, the position of the flat surface with regard to the scale is always the object to be ascertained; and this can be done, as is the case with Erdmann's float, so as entirely to avoid parallax. The level of the liquid's surface is afterwards taken; and the difference between the two readings is the depth required; but if the button be used, the height of the button must be subtracted from that difference.

It is of course obvious that any upward or downward movement of the rod must alter somewhat the level of the surface of the liquid. For small variations thus produced (as, for example, by a depression of two or three divisions) no correction need be made. For larger variations, a factor is easily found by experiment; it is probably the same in every specimen of the instrument, viz. nearly 0.015 division for every division the rod is moved. This correction is perhaps rather better than direct reading.

Remarks on Colorimetry.

The colorimeter has been of late years more extensively used than formerly; but it would probably be much more widely employed if its service were better understood. Thus, for example, a red liquid like a solution of magenta is admirably suited for colorimetric measurement, it having a tint to which the eye readily adapts itself. On the other hand, it is rare to find any one who can accurately estimate yellow. Something thus depends on the eye, and on the employment of the same eye. It must also be borne in mind that very few liquids will stand a dilution of over 20 per cent. without undergoing chemical change. Thus, a very weak solution of magenta differs in actual colour from a strong one. Hence it is obviously necessary to use the first determination as a mere approximation; and, on that as a basis, to alter the strengths of the standard and trial liquid to equality. A second determination is now made, and a still closer approximation obtained

by its means—this process being repeated until there is only a difference of a division or two between the two liquids.

The second approximation will in general be found sufficiently exact. All dilutions should as far as possible have the same age. With regard to the standard tint selected, the operator has in this colorimeter the means of varying his standard to any extent by shifting one opal disk; he can thus work at the particular depth of tint which he finds most suitable to his own eye. Steady accuracy in any particular measurement can generally be obtained by at most a few days' practice.

Turbidities.—In connexion with the Portable Colorimeter, I pointed out* that a black or coloured disk, lowered through a turbid liquid, eventually vanishes, and that the depth at which disappearance takes place is a measure of the amount of turbidity present. In this way, for example, it is easy to estimate the amount of water added to milk. It is obvious, however, that this method admits of quantitative extension to all sorts of precipitates, provided we can find a suitable medium to ensure their suspension as a turbidity, and not in the aggregated state, during a suitable time.

The suspensory liquid I now employ consists of 100 grms. of gelatine, 100 grms. at most of glacial acetate ("acetic acid"), and 1 gram. of salicylate ("salicylic acid") dissolved in a litre of distilled water: this is clarified with a little white of egg, and filtered hot. It remains permanently liquid in the cold, and does not putrefy. It may, if desired, be charged with any special reagent (baric chloride for instance): a volume of the mixture can then be added to a volume of a very weak standard sulphate, and also to a volume of sulphate of unknown strength; by depressing the black buttons, the colorimeters determine the relation between the two. The reacting bodies should in such cases be the same; thus, hydric sulphate should not be compared against potassic sulphate. The key to success in colorimetry is, in fact, equality of condition.

If the precipitant should be alkaline, or an alkaline carbonate, the gelatine solution should first be neutralized and then mixed with more alkali or carbonate. Such solutions as aqueous magnesian chloride and zinc sulphate can then be added, the whole instantly well shaken, and the result compared with a standard effect in the other tube.

Lime can be determined by adding ammonia and ammoniacal oxalate to the suspensory liquid, and then a weak solution of calcic salt.

There is probably no substance incapable of suspension for

* *Loc. cit.* p. 312.

more than half an hour—a period sufficient for thirty comparisons; and most precipitates will refuse to fall for hours, sometimes for days, together. Traces of argentic chloride will remain unprecipitated in this liquid for months. The operator has therefore only to select such a strength of standard precipitate as shall give him not too great an amount to suspend, and an opacity equal to about fifty scale-divisions. If the substance precipitated should be soluble in the solution of gelatine, that solution should be saturated, before use, with the precipitate in question.

The estimation of turbidities will doubtless prove of much value in water-analysis, in field work, in the valuation of pharmaceutical extracts precipitable by water (hitherto an unapproachable subject), in watching the variations in composition of well-water for brewing-purposes, in the systematic examination of the atmosphere's impurities, in Eggertz's carbon process, and in many similar lines of research.

The colorimeter is an instrument admirably adapted for use in comparatively unskilled hands, and especially in those industrial analyses where one class of product is constantly tested by a single person.

LXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 370.]

April 30.—Henry Clifton Sorby, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "A Contribution to the History of Mineral Veins." By John Arthur Phillips, Esq., F.G.S.

In this paper the author described the phenomena of the deposition of minerals from the water and steam of hot springs, as illustrated in the Californian region, referring especially to a great "sulphur bank" in Lake County, to the steamboat springs in the State of Nevada, and to the great Comstock lode. He noticed the formation of deposits of silica, both amorphous and crystalline, enclosing other minerals, especially cinnabar and gold, and in some cases forming true mineral veins. The crystalline silica formed contains liquid-cavities, and exhibits the usual characteristics of ordinary quartz. In the great Comstock lode, which is worked for gold and silver, the mines have now reached a considerable depth, some as much as 2660 feet. The water in these mines was always at a rather high temperature; but now in the deepest mines it issues at a temperature of 157° Fahr. It is estimated that at least 4,200,000 tons of water are now annually pumped from the workings;

and the author discussed the probable source of this heat, which he was inclined to regard as a last trace of volcanic activity.

2. "*Vectisaurus valdensis*, a New Wealden Dinosaur." By J. W. Hulke, Esq., F.R.S., F.G.S.

3. "On the Cudgegong Diamond-field, New South Wales." By Norman Taylor, Esq., of the late Geological Survey of Victoria; communicated by R. Etheridge, Esq., Jun., F.G.S.

The author described in detail the various spots at which diamonds have been found in this locality. They occur in river-drift, associated with gold and other gems. The drifts in the district are at least six in number. The oldest is considered by the author to be Upper Miocene or Lower Pliocene; the next Middle Pliocene; others Upper Pliocene, Pleistocene, and Recent. Between the Middle and Upper Pliocene flows of basalt lava took place, which have sealed up much of the older drifts. Diamonds are found in the oldest drift and, probably by derivation from it, in the newer. Gold, metallic iron, wood, tin, brookite (?), iron-sand, quartz, tourmaline, garnet, pleonast, zircon, topaz, sapphire, ruby, and corundum are also found. The author then considers the question of whether the diamonds are derived from some of the igneous or sedimentary formations (from Upper Silurian to Mesozoic) which have contributed to the drift; and concludes, from a variety of reasons, that the diamonds have been formed *in situ* in the older drift.

4. "On the Occurrence of the Genus *Dithyrocaris* in the Lower Carboniferous, or Calciferous Sandstone Series of Scotland; and on that of a second species of *Anthrapalcæmon* in these beds." By R. Etheridge, Esq., Jun., F.G.S.

LXIX. Intelligence and Miscellaneous Articles.

NOTE ON THE MAGNETIC EFFECT OF ELECTRIC CONVECTION.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Johns Hopkins University,
Baltimore, April 8, 1878.

SOME three years since, while in Berlin, I made some experiments on the magnetic effect of electric convection, which have since been published in the 'American Journal of Science' for January 1878. But previous to that, in 1876, Professor Helmholtz had presented to the Berlin Academy an abstract of my paper, which has been widely translated into many languages. But, although Helmholtz distinctly says "Ich bemerke dabei, dass derselbe den Plan für seine (Rowland's) Versuche schon gefasst und vollständig überlegt hatte, als er in Berlin ankam, ohne vorausgehende Einwirkung von meiner Seite," yet nevertheless I now find that the experiment is being constantly referred to as Helmholtz's

experiment—and that if I get any credit for it whatever, it is merely in the way of carrying out Helmholtz's ideas, instead of *all* the credit for ideas, design of apparatus, the carrying out of the experiment, the calculation of results, and *every thing* which gives the experiment its value.

Unfortunately for me, Helmholtz had already experimented on the subject with *negative* results ; and I found, in travelling through Germany, that others had done the same. The idea occurred in nearly the same form to me eleven years ago ; but as I recognized that the experiment would be an extremely delicate one, I did not attempt it until I could have every facility, which Helmholtz kindly gave me.

Helmholtz kindly suggested a more simple form of commutator than I was about to use, and also that I should extend my experiments so as to include an uncoated glass disk as well as my gilded vulcanite ones ; but *all else I claim as my own, the method of experiment in all its details, the laboratory work, the method of calculation—indeed every thing connected with the experiment in any way, as completely as if it had been carried out in my own laboratory 4000 miles from the Berlin laboratory.*

Yours truly,
H. A. ROWLAND.

ON ELECTRIC BOUNDARY LAYERS. BY PROF. HELMHOLTZ.

In all cases in which two contiguous bodies have different values of the electric-potential function, there must be along the common boundary between them a double layer of positive and negative electricity, the moment of which (taking this expression in the same sense as in the phrase “magnetic moment”), multiplied by 4π , calculated for unit surface, is equal to the difference of potential-function on the one side and on the other of the double layer. Now, as the value of the moment is equal to the electric density of the positive E multiplied by the mean value of the distance between the two layers, this distance cannot become vanishingly small without the density with a given difference of potential becoming infinitely great. But the work done, in the formation of such a double layer, against the electrostatic forces is $=PE$, if E denotes the amount of positive electricity on the unit of surface, and P the difference of potential on the two sides of the double layer. Since, with the distance h between the two layers,

$$4\pi Eh = P,$$

the value of the work is

$$\frac{1}{2} PE = \frac{1}{8\pi h} P^2,$$

and would therefore become infinite for vanishing h . From this Sir William Thomson has already calculated a limit for the distance between the double layer at the galvanic tension between copper

and zinc, according to which calculation it must be more than $\frac{1}{3} \times 10^{-7}$ millim. With this agree F. Kohlrausch's experiments on the capacity of galvanically polarized platinum surfaces for very weak charges, from which the distance comes out equal to the 2475000th part of a millimetre, if the potential-difference be assumed to be equally divided between the two plates.

The author showed that the laws of the flow of water through capillary tubes and porous diaphragms occasioned by electric currents, as ascertained by G. Wiedemann and Quincke, and the laws of the electric tension excited by the flowing of water, between the beginning and the end of the course of the stream, discovered by the latter observer, can all be deduced from the hypothesis that a difference of electrical potential exists between the sides of the vessel and the liquid (which M. Quincke also assumed, and supported by many experiments), and that it is the part of the double layer falling in the water that both yields to the electric attractive forces on the tube being traversed by an electric current, and is also taken along by the introduced motion of the water. The boundary layer of the liquid must be assumed to be at rest against the sides of the tube, as in Poisseuille's theory of the flow of liquids in capillary tubes. In a series of cases the data supplied suffice for the calculation of the electric moment of the part of the double layer that falls in the liquid (in which calculation the opposite electricities must be assumed to be combined in the bounding surface). The values then obtained do not exceed those with which we are acquainted from the galvanic tensions between metals.

Thus M. Wiedemann's experiments on the electric conveyance of sulphate-of-copper solution through clay diaphragms give the moment of the electrical layer in the liquid as equal to 2.4 Daniells. M. Quincke's experiments on the height to which water, conveyed by electricity, ascends in glass tubes give 3.9 Daniell's; and his experiments on the electric tensions which arise when very dilute salt-solutions are driven through clay diaphragms give 1.9-2.7 Daniells. As the electromotive force between potassium and platinum amounts to about 3.4 Daniells, all the above-mentioned numbers lie within or but little beyond the limits of the observed differences of potential between metals.

The assumption that the extreme boundary layer of the liquid adheres immovable to the sides of the vessel was founded upon the determinations made by M. Quincke of the heights of ascent of electrically carried liquid in cylindrical glass tubes, according to which they are inversely proportional to the square of the radius. This results from the theory only on the assumption that no sliding of the boundary layer occurs. The calculation was accomplished even for those cases in which a cylindrical thread is placed in a cylindrical tube; and it showed tolerable accordance with the observations, so far as could be expected with experiments so subtle and so disturbed by manifold influences.—*Monatsbericht der kön. preuss. Akademie zu Berlin*, Feb. 1879, pp. 98-200.

A THEORETIC AND EXPERIMENTAL DEMONSTRATION OF THE DEFINITION, "THE TEMPERATURE OF A BODY IS REPRESENTED BY THE LENGTH OF THE THERMAL OSCILLATION OF ITS MOLECULES." BY R. PICTET.

If we admit that *heat* is only the manifestation pure and simple of the molecular forces with which the constituent particles of bodies are endowed, we must necessarily admit also that the mechanical work taken up by the thermal motion must displace the particles from their position of equilibrium and make them move in trajectories of an elliptic form, the amplitude of which will be proportional to the work consumed.

At absolute zero there is no oscillation, the cohesion is maximum; at a certain temperature, fixed for each body, the oscillation will be maximum, and the body, being disaggregated, will dissolve; the molecules will be sufficiently apart to be out of the conditions of stable equilibrium.

In this hypothesis absolute contact of material particles is rendered impossible by the action of the æther; for we admit that the attraction of matter for the æther does not follow the same law as the attraction of matter for matter: for short distances the attraction of matter for the æther prevails over the attraction of matter for itself.

Under these conditions the repulsive forces are useless; there would exist in nature only attractive forces.

On these bases let us consider the action produced by external work supplied to a body supposed at absolute zero. Each molecule will begin to vibrate and oscillate from an extreme exterior position to another position, an interior limit. The evident result of this molecular motion will be to increase the volume of the body in proportion to the mean length of the oscillations of its elementary particles.

The *coefficient of dilatation* will therefore be in accordance either with the number of molecules contained in the body, or with the volume in which those molecules are contained, or, lastly, with the physical forces involved in thermal motion.

Now the two following postulates can be admitted:—

The laws of the attraction of matter for matter are absolutely general and universal.

The phenomena of the disaggregation of bodies are subject to those laws.

These being admitted, let N be the number of molecules contained in unit length of a solid body, l' and l the lengths of oscillation corresponding to the temperatures t' and t ; let α be the coefficient of dilatation of the solid body. We shall evidently have the following equality:—

$$\int_t^{t'} N dl = \int_t^{t'} \alpha dt.$$

Now N is defined by the density and atomic weight of the solid body.

In 1 cubic metre there are $\frac{d}{p}$ molecules, calling the density d , and the atomic weight p . If we wish to get the number of the molecules N (that is to say, the number of molecules contained in the linear unit or the edge of the cube), we have

$$N = \sqrt[3]{\frac{d}{p}}.$$

Taking α for the measured lengthening between zero and 100°C. , we get directly the relation

$$l_{100} - l_0 = \frac{\alpha}{\sqrt[3]{\frac{d}{p}}}.$$

Such is the value of the augmentation of the thermal wave-length when the temperature passes from zero to 100° .

Now, if the attraction of matter for matter obeys a general law, every solid molecule will divide into two or more liquid molecules when the oscillations have become equal to a certain maximum, constant for all bodies.

We have therefore to verify two physical laws which are the necessary consequence of these deductions:—

1st. *The higher the fusion-temperature of a solid, the shorter must the molecular oscillations be.*

2nd. *As the fusion-temperatures of solids correspond to equal lengths of oscillation, the product of the oscillation-lengths into the fusion-temperatures must be a number constant for all solids.*

These two laws are verified with as much exactness as the experimental determinations of the various elements entering into the equations permit.

The following Table comprises the metals of which the coefficients of dilatation are known with sufficient accuracy:—

Table of the Thermal Wave-lengths of Solids, and of the Product of their Multiplication by the Fusion-temperatures*.

Names.	Atomic weights.	Densities.	Values of α .	Wave-lengths.	Fusion-temperatures. $273^\circ +$	Products. $t \times \frac{\alpha}{\sqrt[3]{\frac{d}{p}}}$
Selenium ...	39.75	4.30	0.00368	0.007725	217	3.7854
Lead	104	11.35	0.0028657	0.005382	335	3.272
Zinc	32.7	7.19	0.002942	0.004873	450	3.523
Silver	54	10.60	0.00193	0.003077	977	3.841
Copper	31.75	8.9	0.001715	0.0026215	1050	3.468
Gold	98	19.258	0.001466	0.0025205	1100	3.459
Iron	28	7.79	0.0011717	0.0017805	1600	3.34
Platinum ...	98.5	21.53	0.0008842	0.001467	1700	3.59

* The wave-lengths are in the inverse ratio of the fusion-temperatures; and the products are sensibly constant.

We may therefore regard the two postulates above indicated as correct.

The temperature is really represented by the length of oscillation of the molecules of solid bodies.

Analogous equations connect the elements of volatile liquids when compared at their boiling-points.—*Comptes Rendus de l'Académie des Sciences*, April 28, 1879, t. lxxxviii. pp. 855-857.

ON OZONE AND THE ELECTRIC EFFLUVIUM. BY M. BERTHELOT.

1. The following are some experiments selected from those which I have made in the course of my researches upon persulphuric and hyperoxygenated acids, experiments the results of which appeared to me worth making known.

2. First, of the combination of oxygen with hydrogen. I have found that these two gases, mixed in the proportion of 2 volumes of hydrogen to 1 of oxygen, do not combine under the influence of the effluvium, even at the end of several hours, either in concentric sealed glass tubes*, or in a tube surrounded by a lamellar spiral of platinum† and placed over mercury; in my trials the tension was nearly that developed across air by sparks of 7 or 8 centims. length in operating with an induction-coil furnished with a condenser. No doubt, by progressively increasing the tensions up to approximately those which produce disruptive discharges, the formation of water would be provoked. But it appeared to me of interest to prove that that formation does not take place with such tensions as the preceding, and under conditions where the portion of ozone formed is very considerable.

The resistance of hydrogen to combination under these conditions is the more remarkable, as they are precisely those under which oxygen combines with metals, with sulphurous acid, arsenious acid, iodine, and even with nitrogen, although this last reaction demands considerably stronger electrical tensions than the others.

Under these conditions, moreover, the vapour of water is not decomposed by the effluvium, nor does oxygen combine with water to form oxygenated water.

3. These phenomena contrast with those which I have observed on carbonic acid. In fact, the oxide of carbon and oxygen, mixed in a test-tube over mercury in the proportion of two volumes of the one to one volume of the other, combine under the influence of similar electric tensions to the preceding. After twelve hours there remained only 8 per cent. of oxide of carbon and 2 per cent. of oxygen. One part of the latter had been absorbed by the mercury; and a portion (about 5 hundredths) of the oxide of carbon had co-operated in the formation of the brown suboxide, C_3O_4 .

This incompleteness of the reaction is not less manifest in the

* *Annales der Chimie et de Physique*, 5 série, t. xii. p. 466.

† *Ibid.* t. x. p. 79.

presence of an excess of oxygen. For example, on mixing, over mercury, equal volumes of oxide of carbon and oxygen, I found after some hours 93 per cent. of the oxide of carbon changed into carbonic acid, 5 per cent. into suboxide, and 2 per cent. unaltered. There remained 42 per cent. of free oxygen, including a little ozone. The presence of an excess of oxygen, therefore, does not determine the total combination of the oxide of carbon.

Reciprocally, it does not prevent incipient decomposition of carbonic acid, as I have specially ascertained. Nay, more: in a mixture of equal volumes of carbonic acid and oxygen I found, after twelve hours, in a system of two concentric tubes, 5 per cent. of the gas decomposed into carbonic oxide and oxygen. This oxygen contained a strong dose of ozone (or of percarbonic acid).

These results establish the existence of the two opposite reactions provoked by the effluvium, and consequently that of the chemical equilibria determined thereby; but it has not been possible to extend them on both sides to the same limit, on account of the secondary reactions, such as the formation of the suboxide of carbon and the absorption of oxygen by mercury.

4. The decomposition of carbonic acid by the effluvium, effected in a space free from mercury and oxidable substances, gives rise to special phenomena worthy of our interest; for they lead one to suspect the existence of percarbonic acid. In fact, in an experiment, after twelve hours of the effluvium acting upon a gas enclosed in the annular space of the concentric tubes hermetically sealed which I am accustomed to employ, I found 16 hundredth parts of carbonic acid decomposed. The gas which was formed attacked mercury and oxidable bodies with extreme violence.

If the oxidating portion of this gas were regarded as ozone, the quantity of that substance would amount to 30 per cent. of the oxygen liberated in one experiment, and up to 41 per cent. in another—enormous proportions, and much higher than those produced with pure oxygen*.

It would be very interesting to isolate the oxidating material formed in this reaction; but when one essays to eliminate the carbonic acid and the oxide of carbon contained in the preceding mixture, the oxidating gas is destroyed by the reagents employed, which does not permit its isolation. This gas might be equally regarded either as oxygen very rich in ozone, or as containing a strong dose of percarbonic acid, C_2O_6 ; but I have not succeeded in discovering any proper character to distinguish the latter compound from ozone mixed with carbonic acid.—*Annales de Chimie et de Physique*, May 1879, sér. 5, t. xvii. pp. 142–144.

* These proportions are relative to the oxygen produced by the decomposition of carbonic acid—which oxygen formed only 8 hundredths of the volume of the whole mixture in one experiment, 5 hundredths in the other.

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END OF THE SEVENTH VOLUME.

Fig. 6.

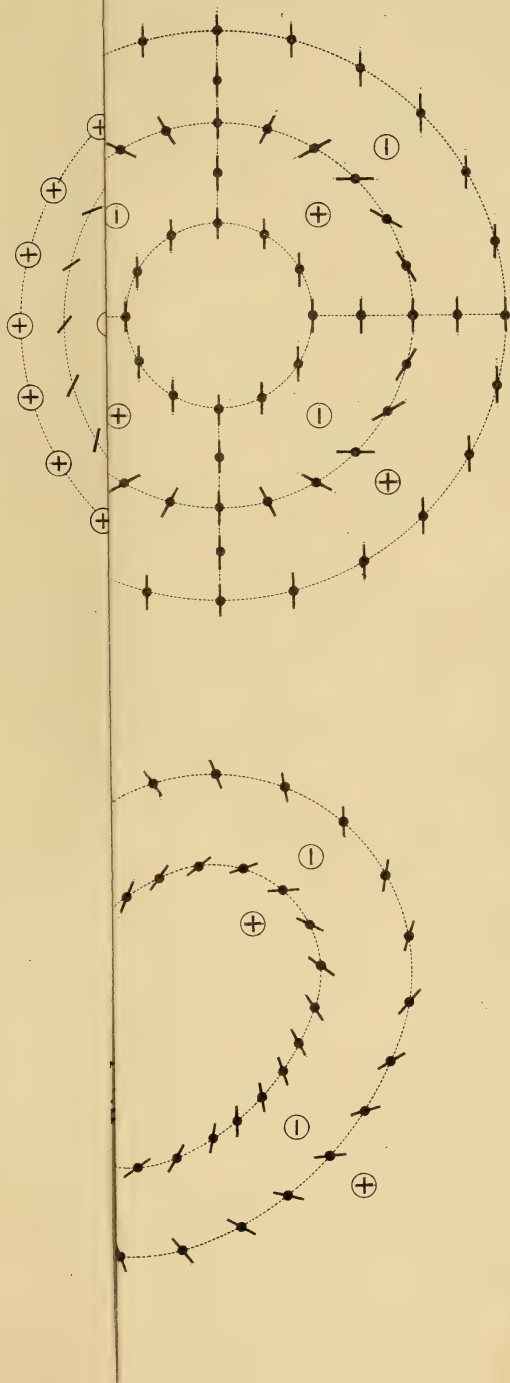


Fig. 1.

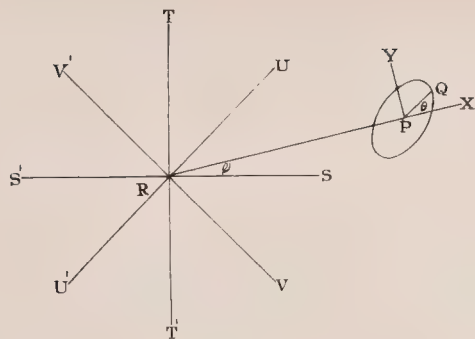


Fig. 6.

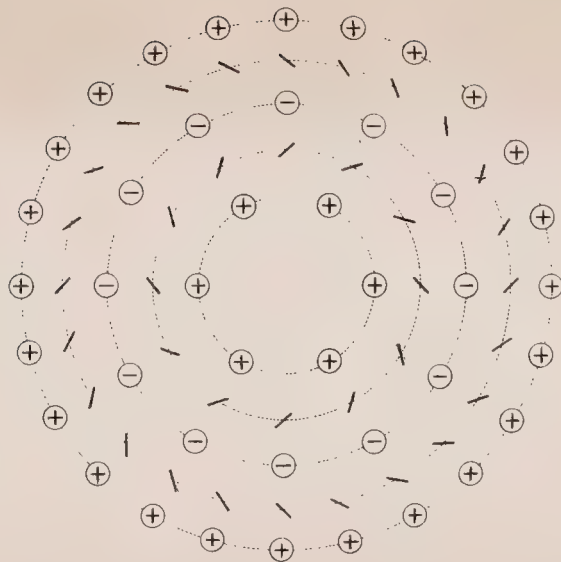


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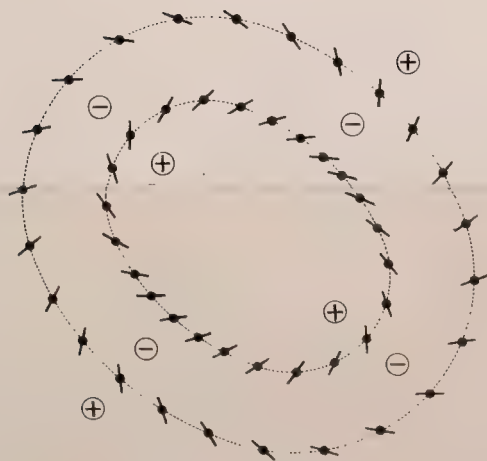


Fig 8.

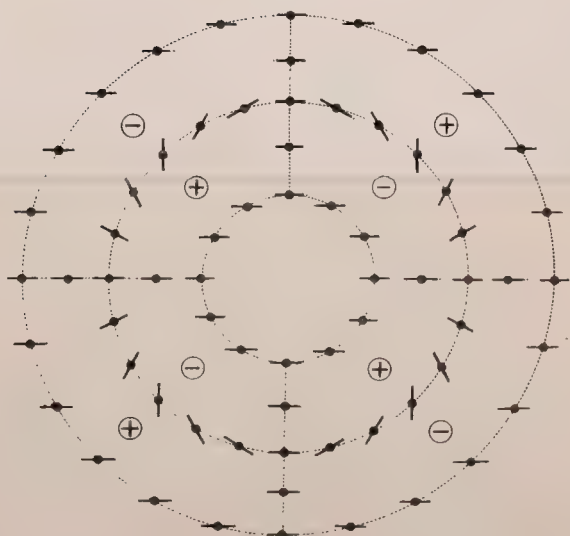


Fig: 2.

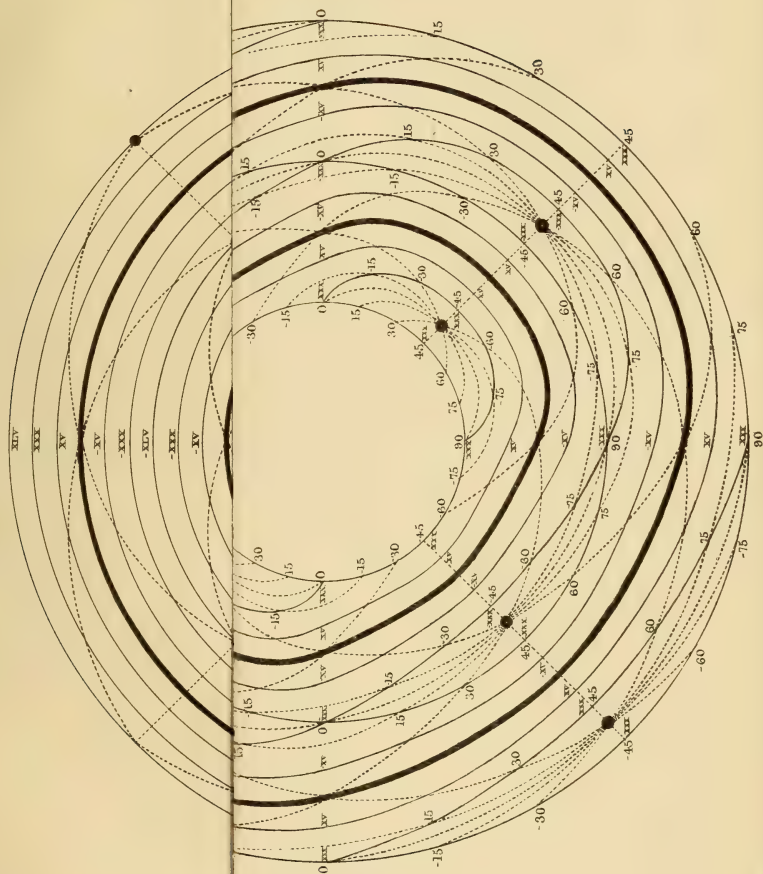


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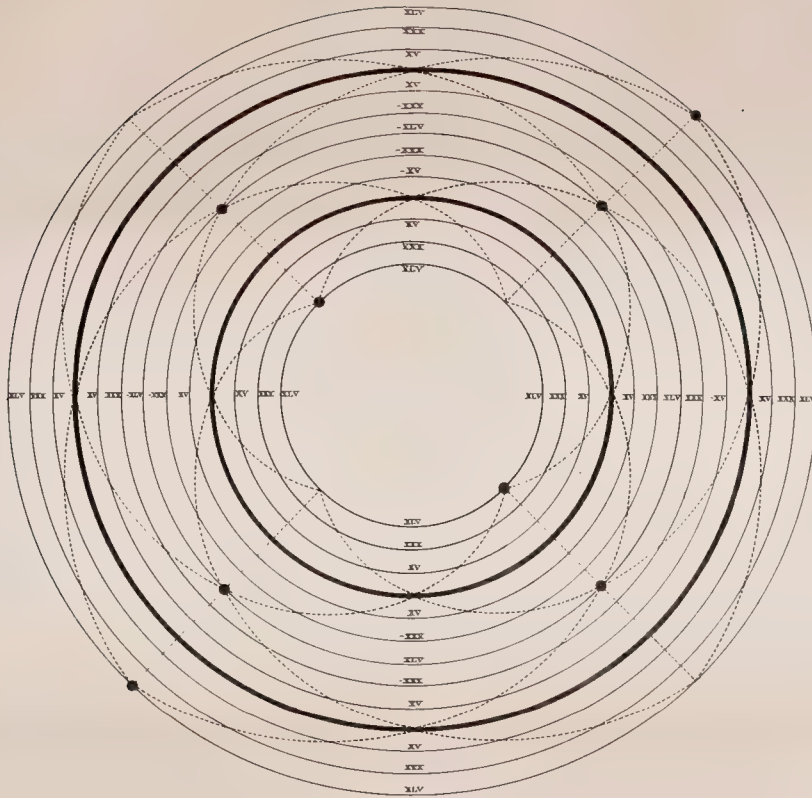


Fig. 3.

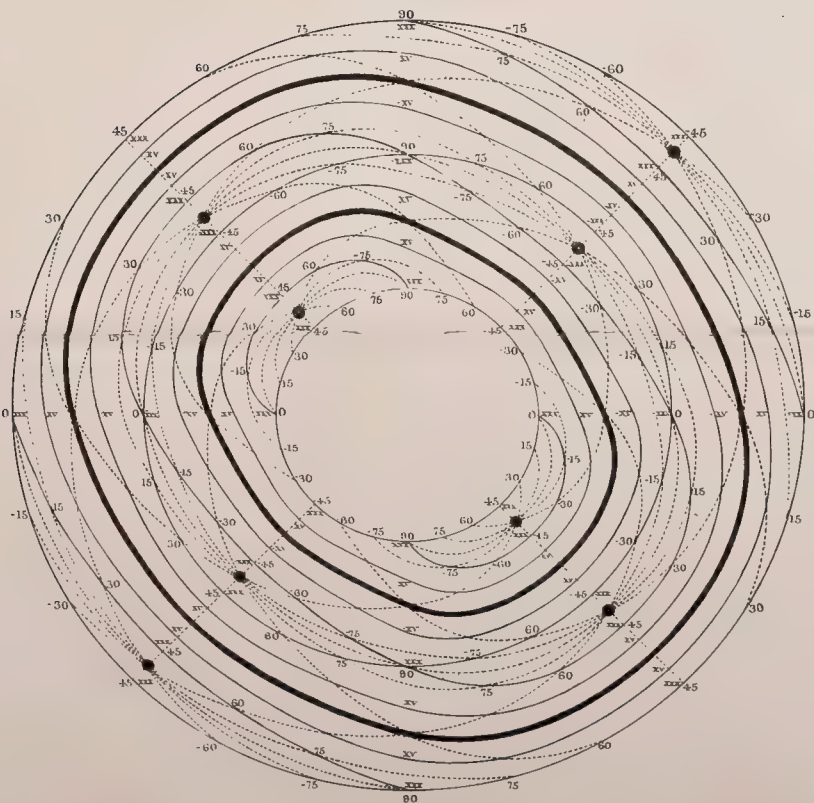




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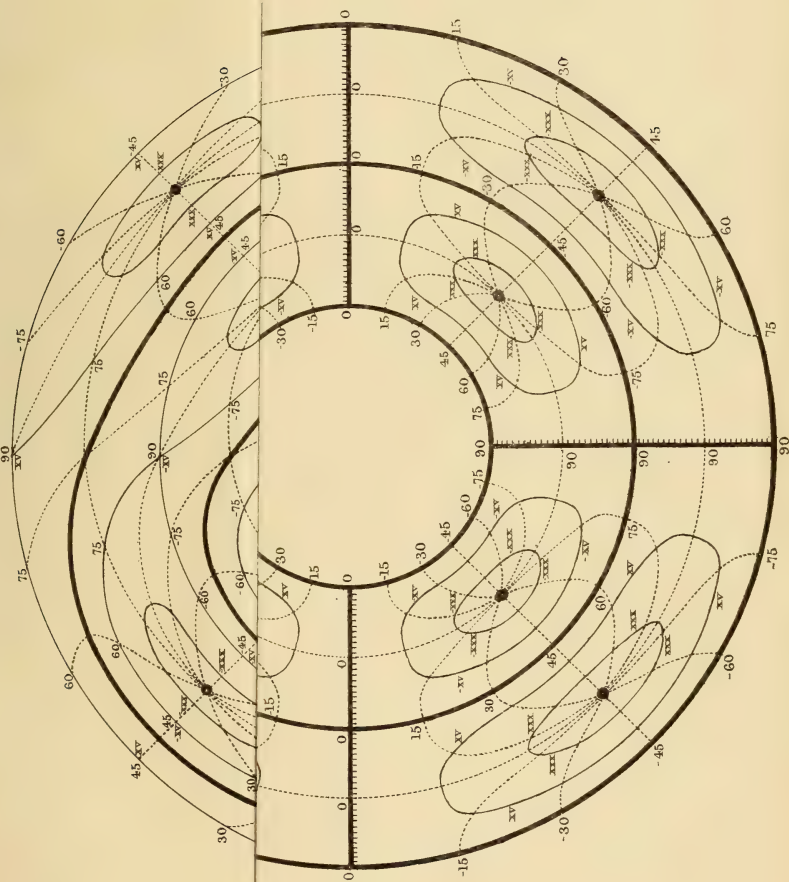
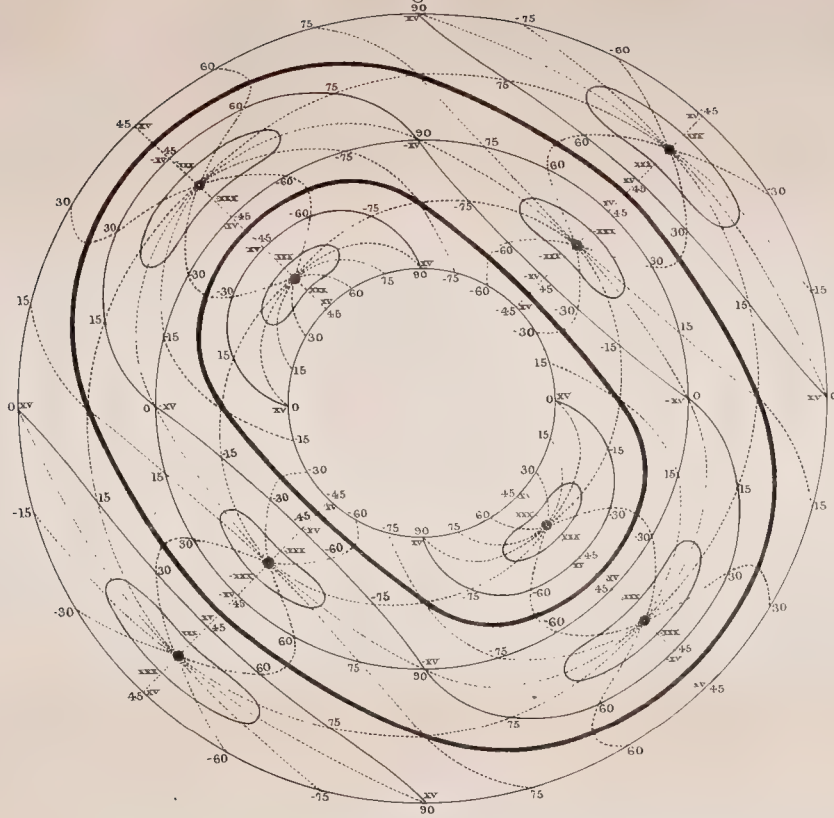


Fig. 4.



F 1 g. 5.

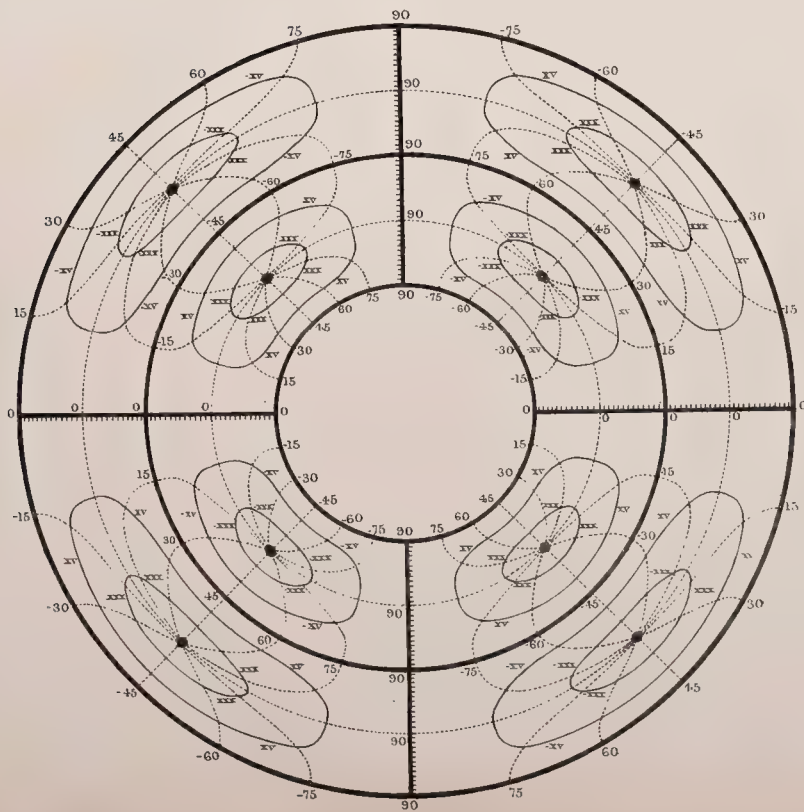




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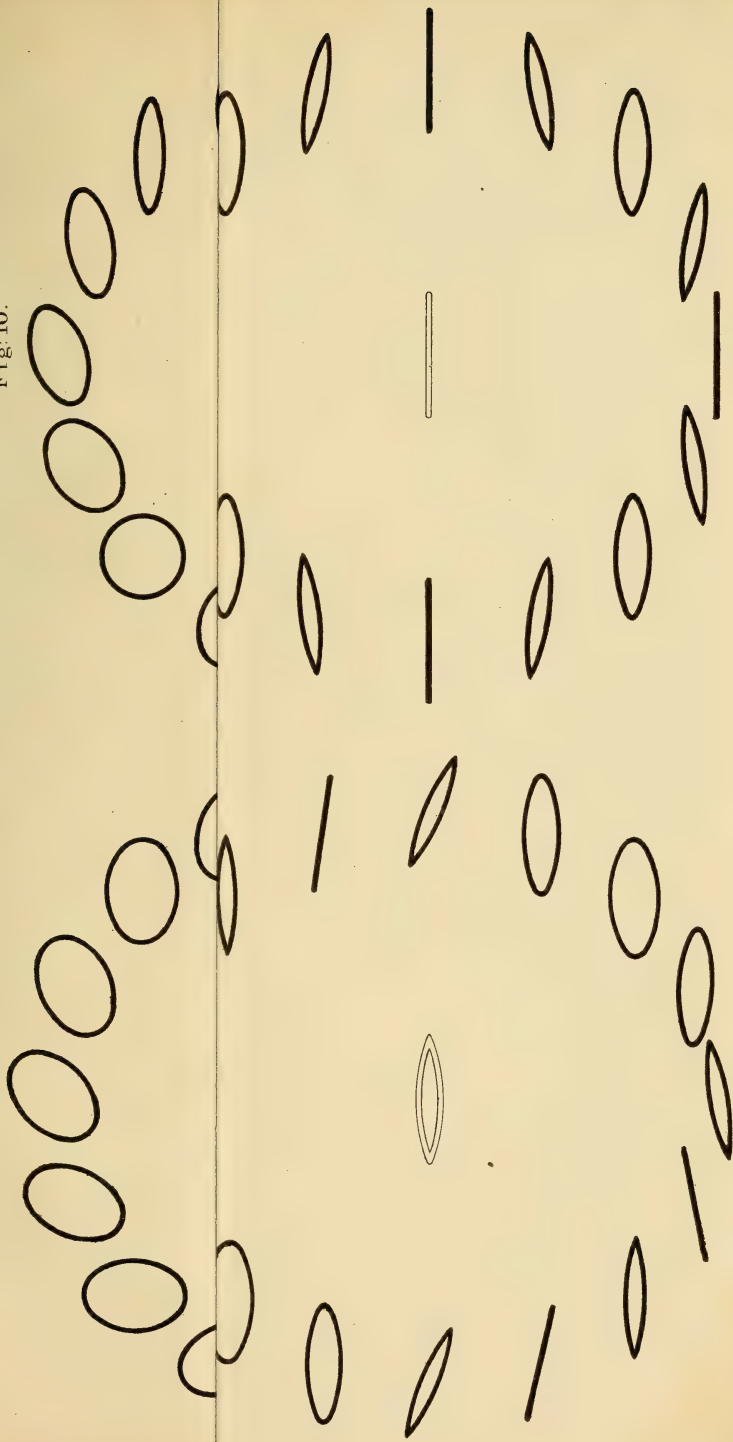


Fig: 10.

Phil. Mag. S 5 Vol. 7 Pl. IV.

Fig 9.

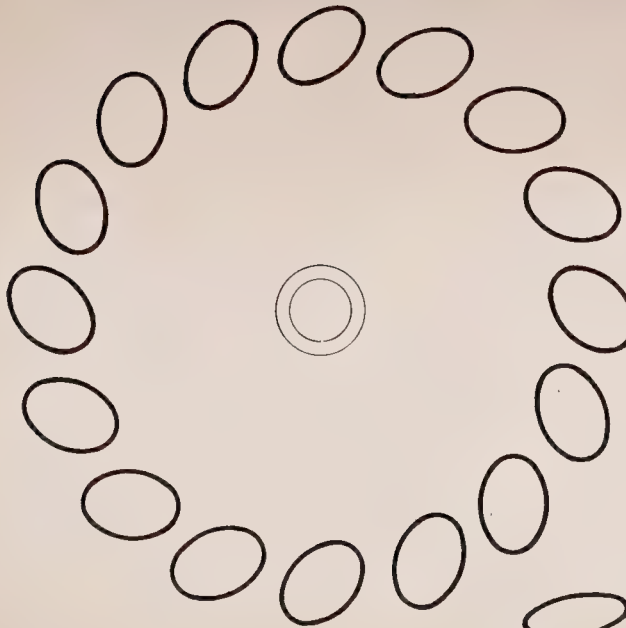


Fig 10.

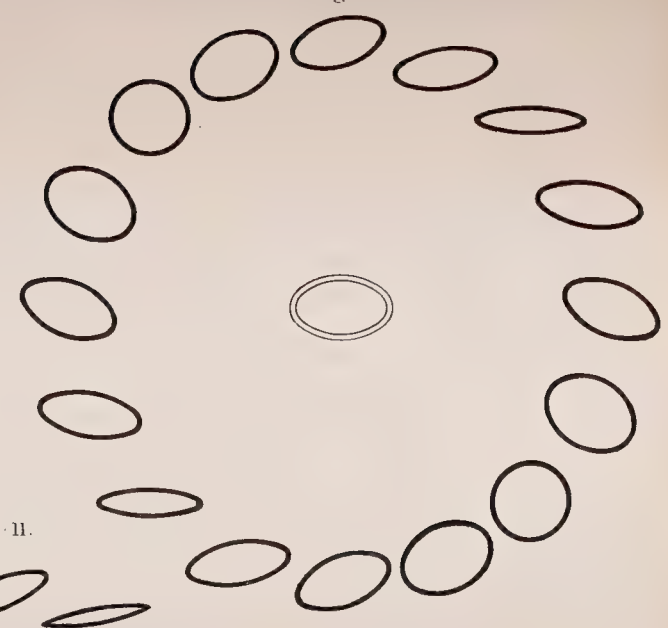


Fig 11.

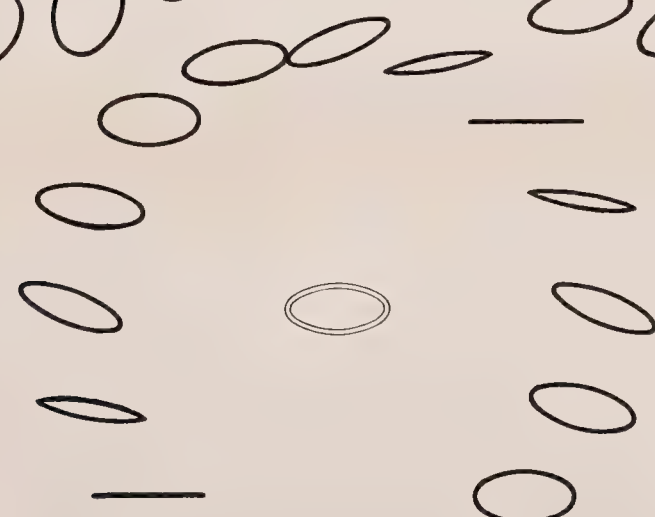


Fig 12.

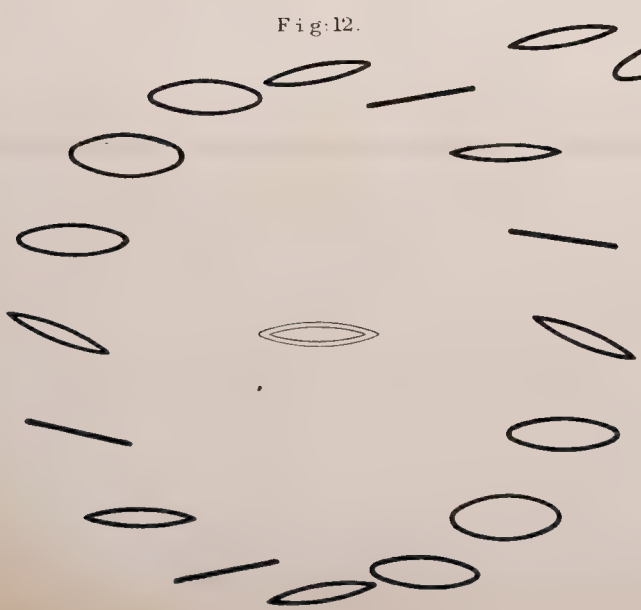
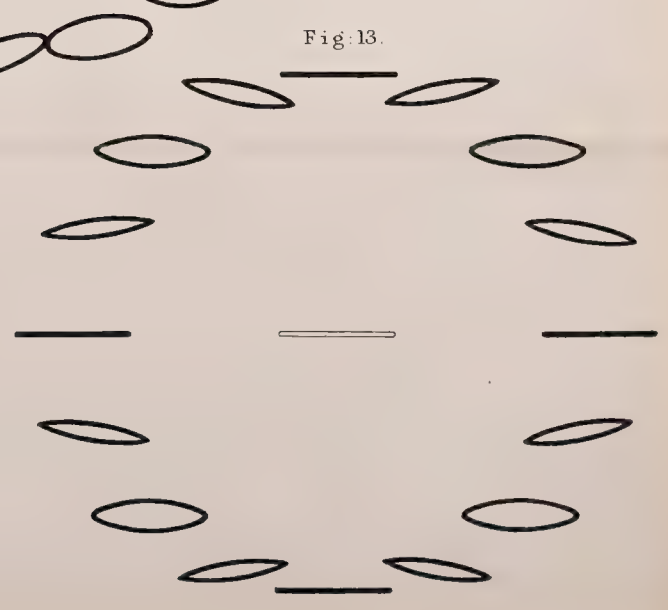


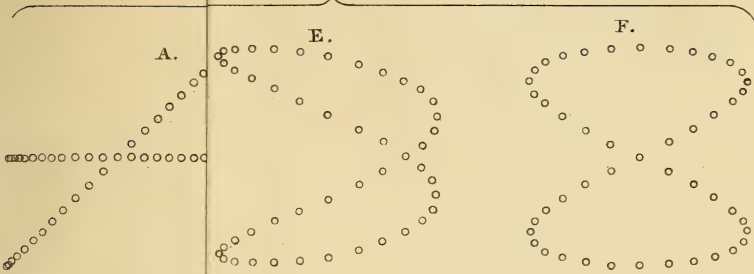
Fig 13.



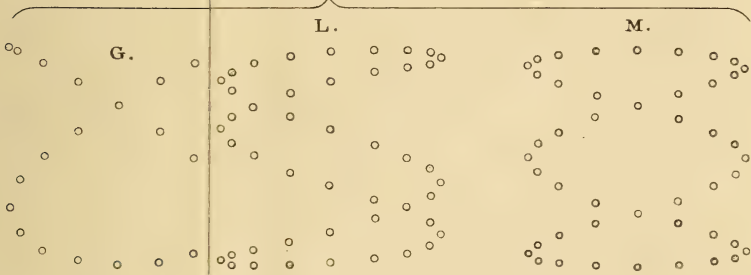


(and Ayrton.)

1 to 2.

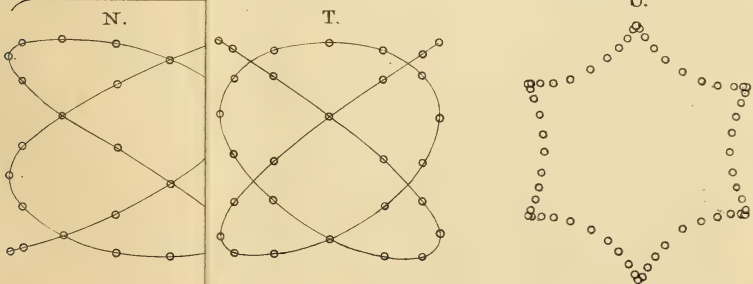


1 to 3.



4 to 5.

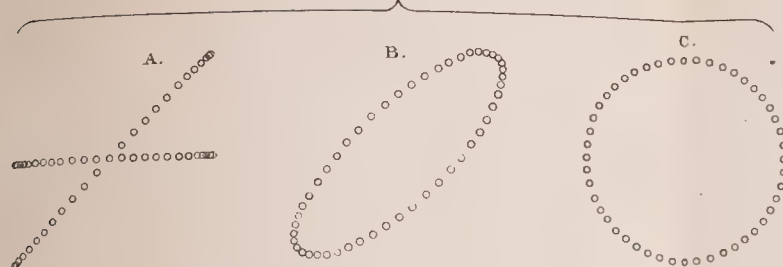
1 to 1 added to 5 to 5.



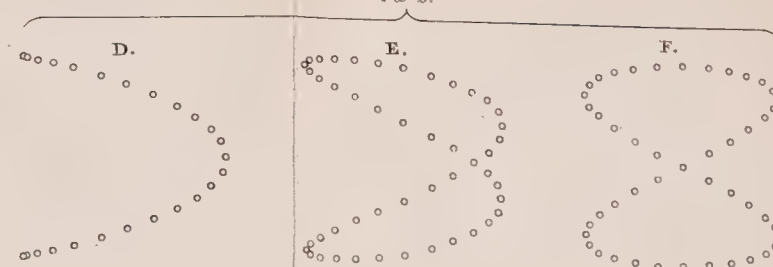
ion of the path.

THE MUSIC AND COLOUR AND VISIBLE MOTION. (*Professors Perry and Ayrton.*)

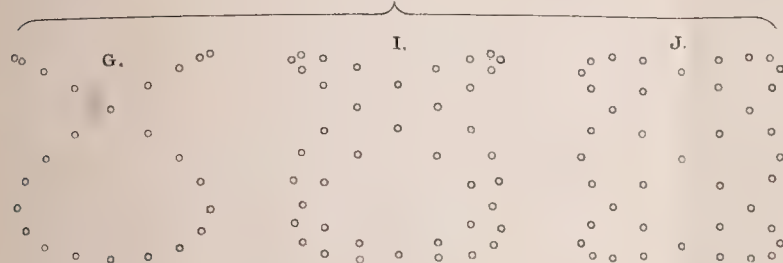
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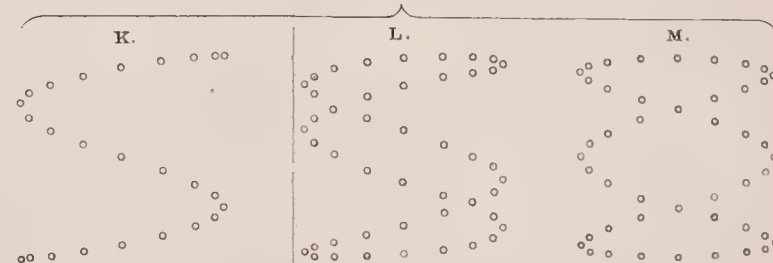
1 to 2.



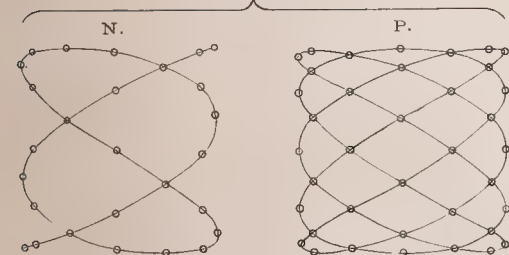
2 to 3.



1 to 3.



3 to 5.



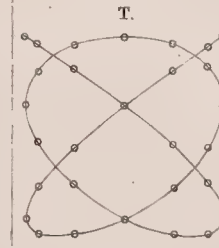
1 to 4.



2 to 5.



4 to 5.



1 to 5 added to 5 to 5.



The distances between the small circles correspond with equal times of description of the path.

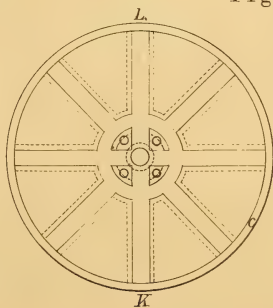
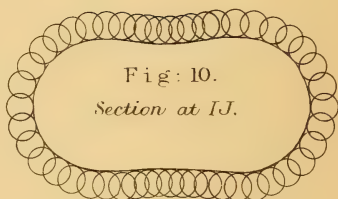
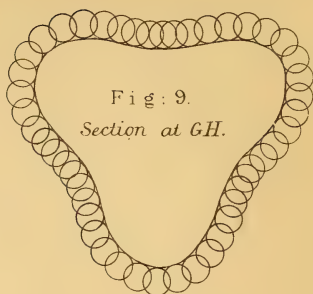
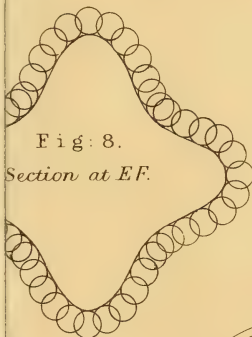
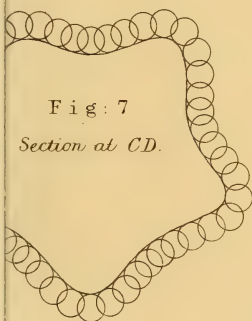
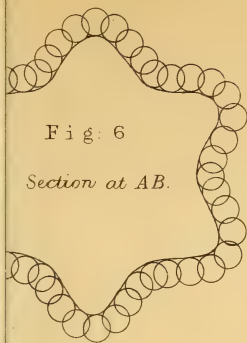


Fig. 13.

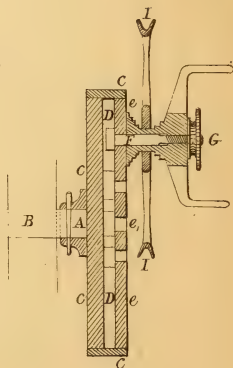


Fig: 1.

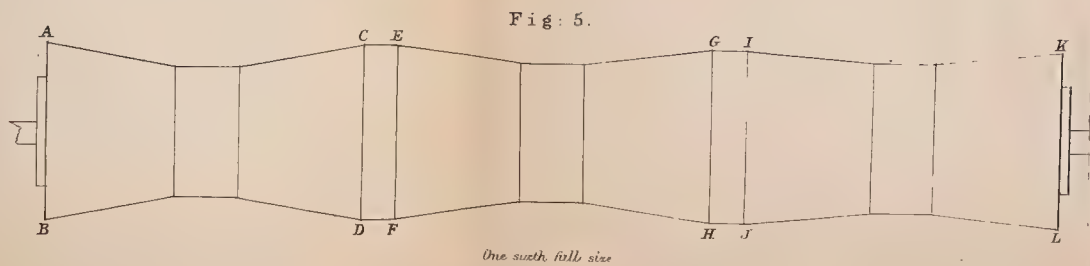
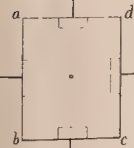
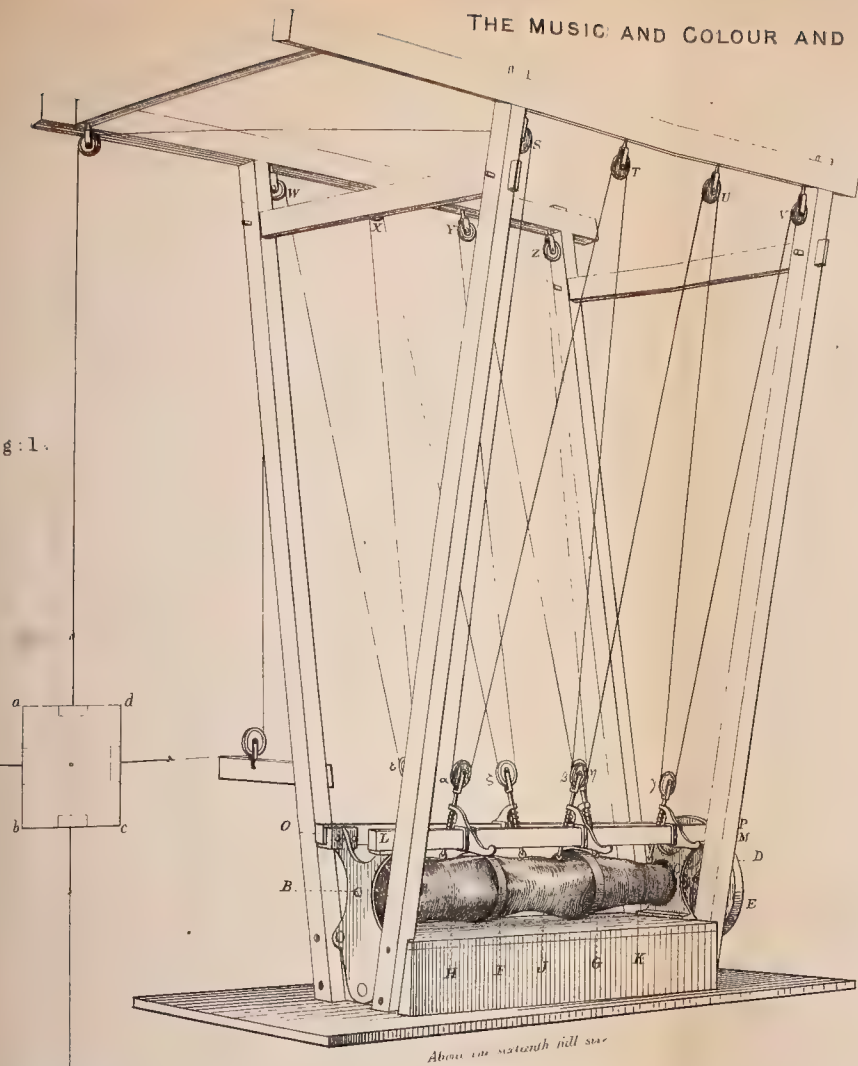


Fig: 5.

One sixth full size

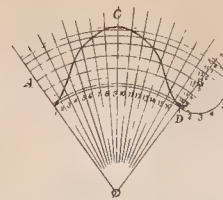


Fig 2

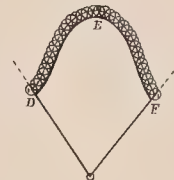


Fig: 3.

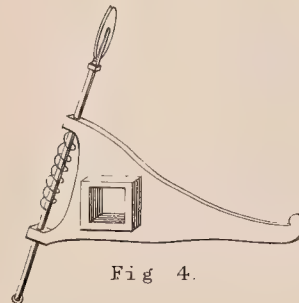


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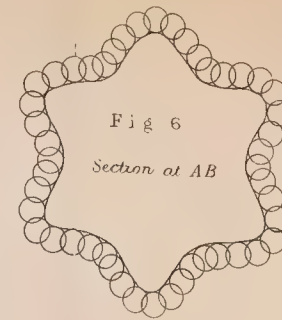


Fig 6
Section at AB

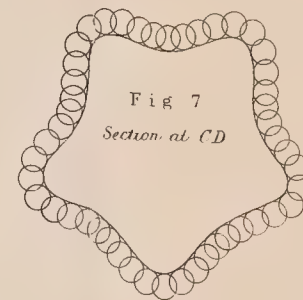


Fig 7
Section at CD

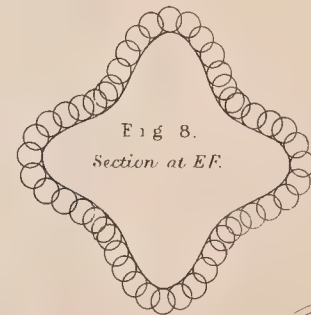


Fig 8.
Section at EF.

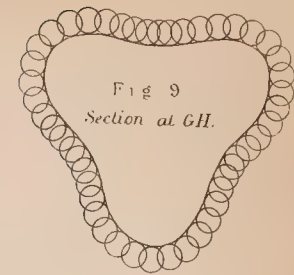


Fig 9
Section at GH.

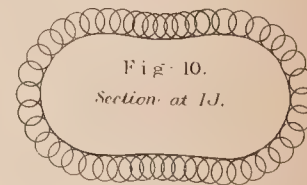


Fig: 10.
Section at IJ.



Fig 11.
Section at KL,
Centre of Roller

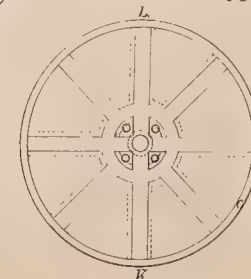
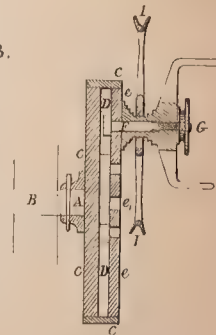


Fig: 13.





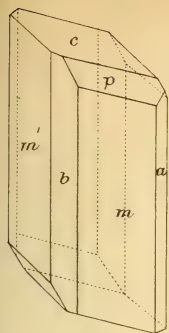


Fig. 1.

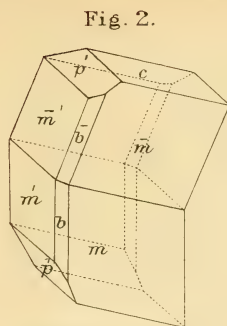


Fig. 2.

Nitroso-terpene type 1.

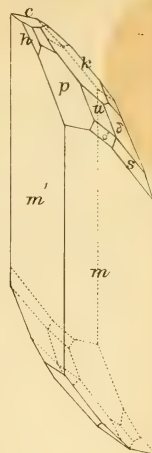


Fig. 3.

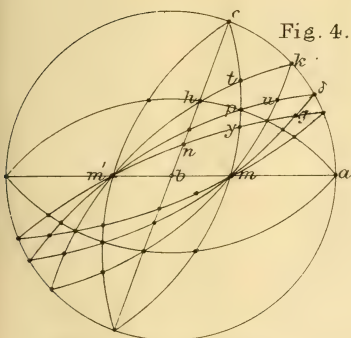


Fig. 4.

Nitroso-terpene type 2.

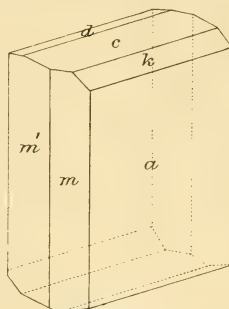


Fig. 5.

Nitroso-terpene type 2.

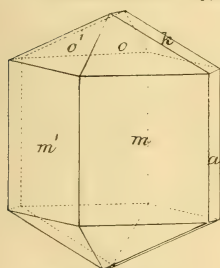


Fig. 6.

Terpene-hydrate.

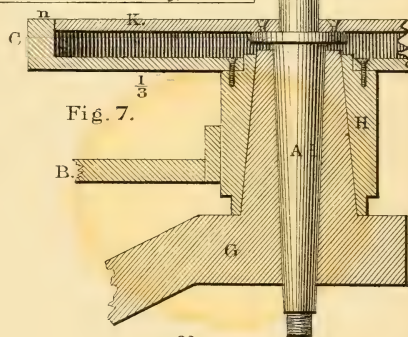


Fig. 7.

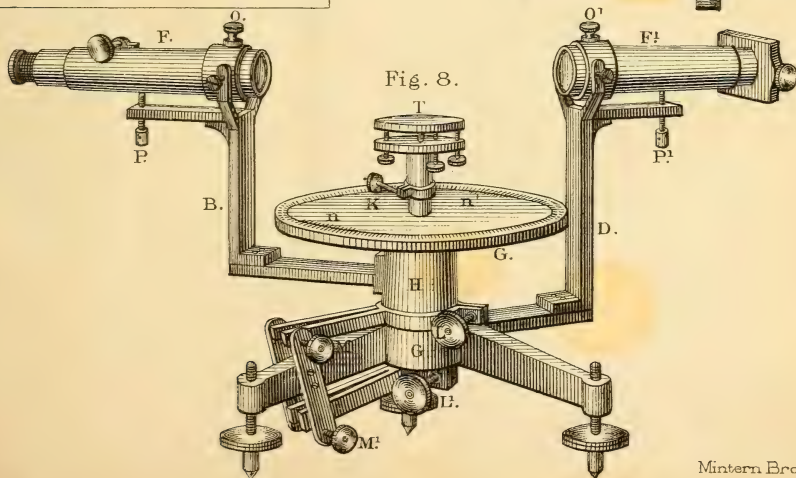


Fig. 8.



Fig. 1.

Fig. 3.
at sixe

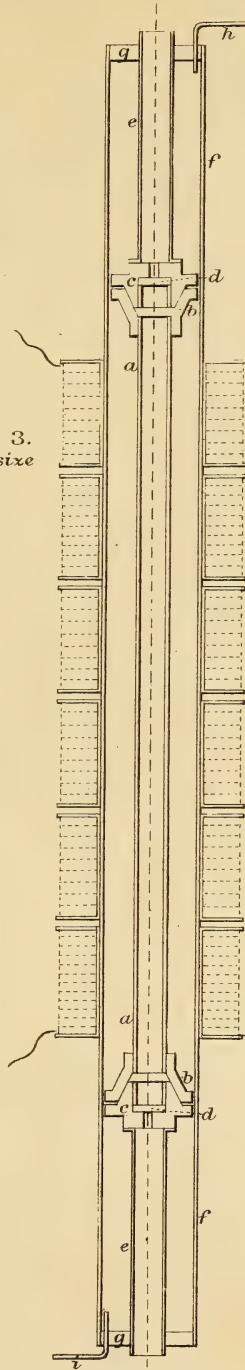
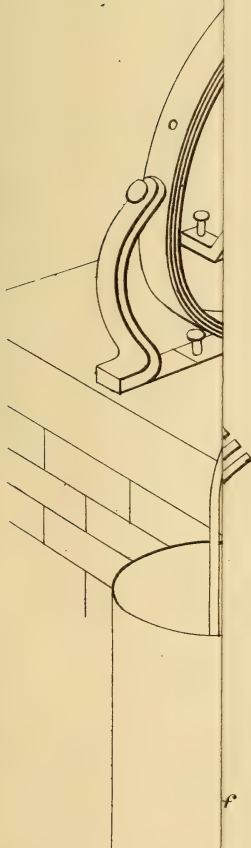


Fig. 1.

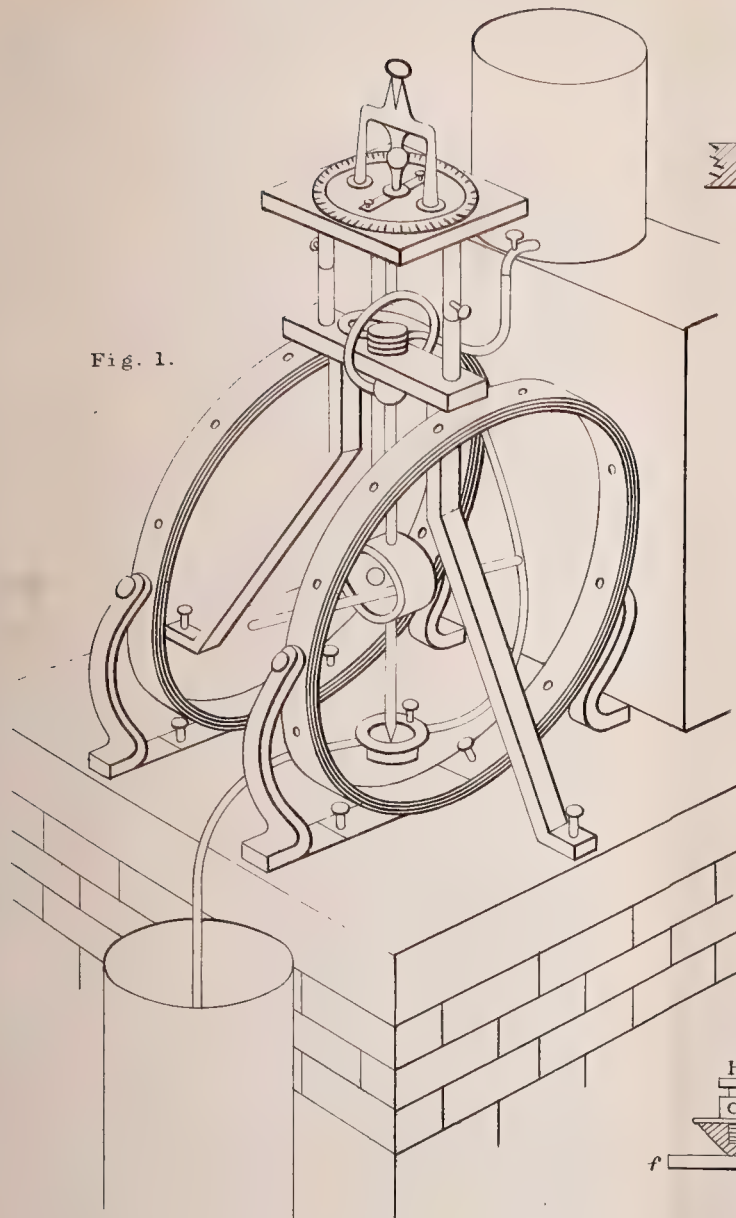


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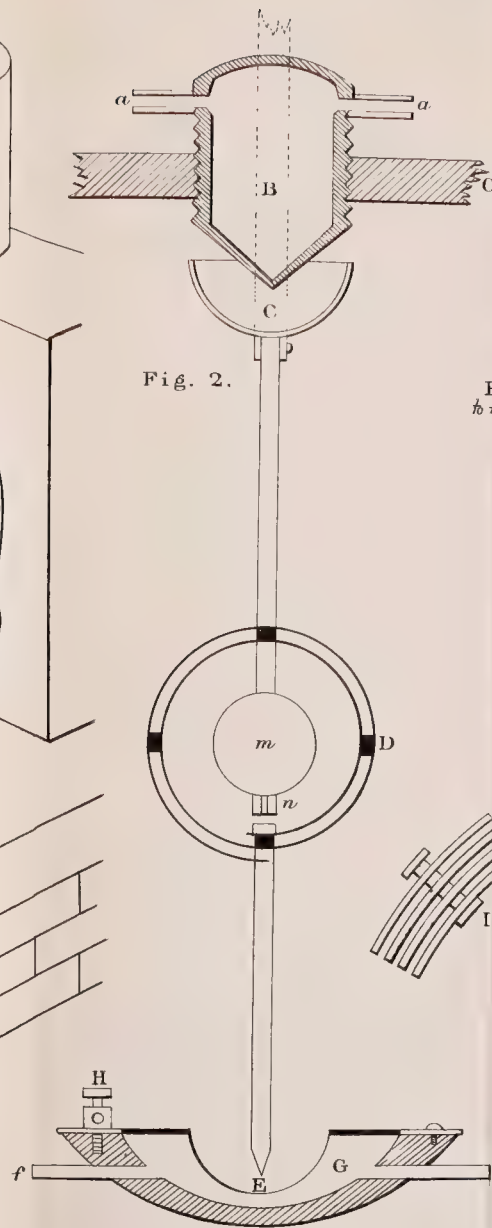
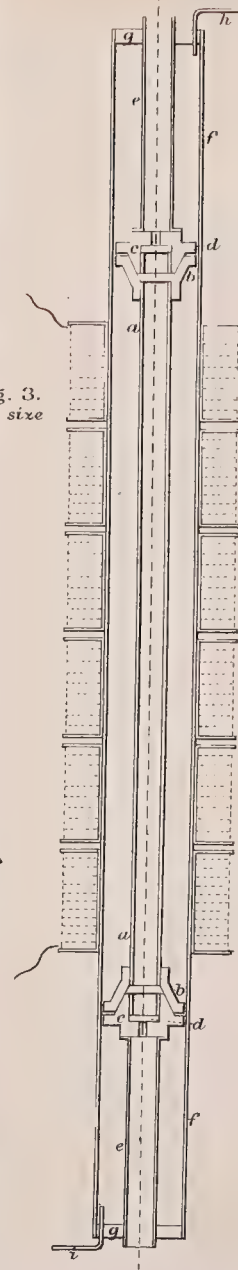
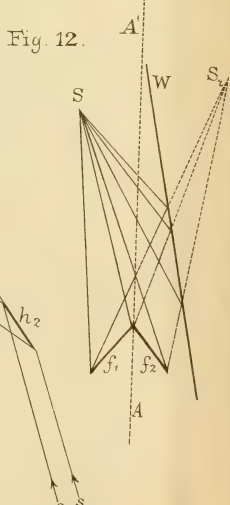
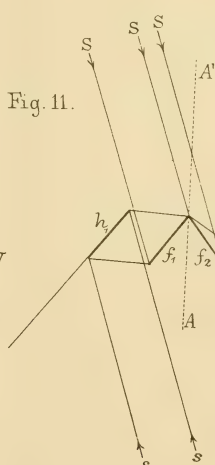
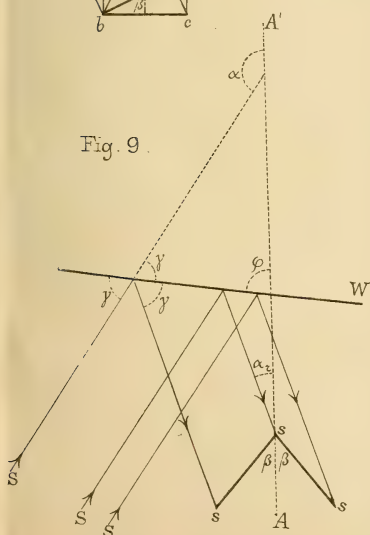
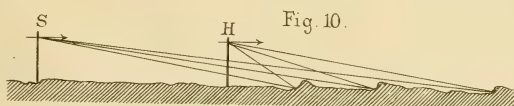
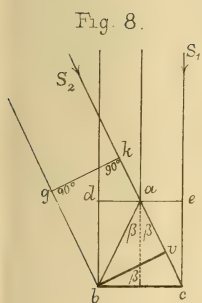
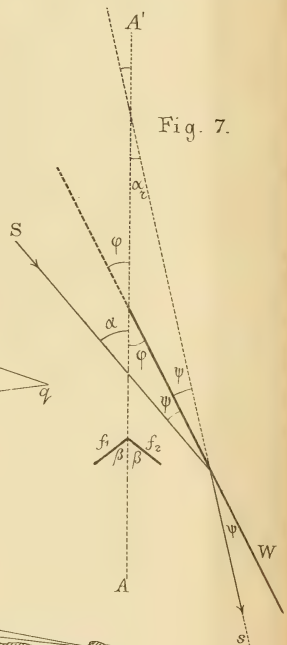
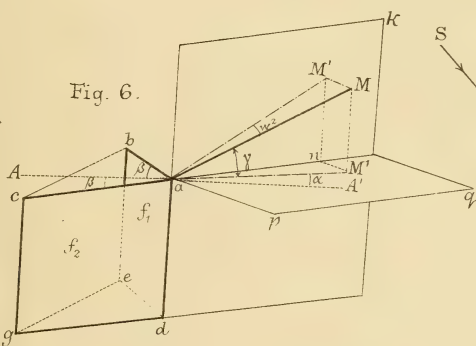
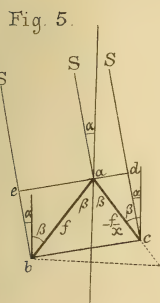
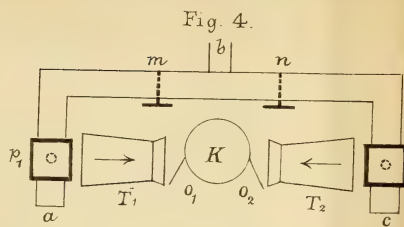
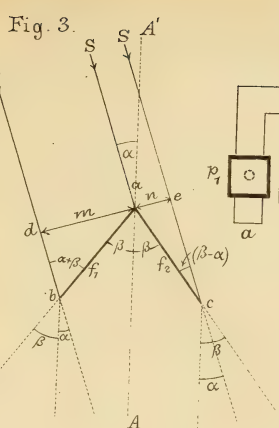
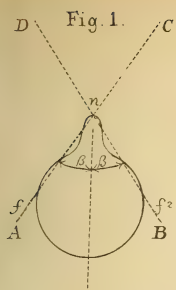
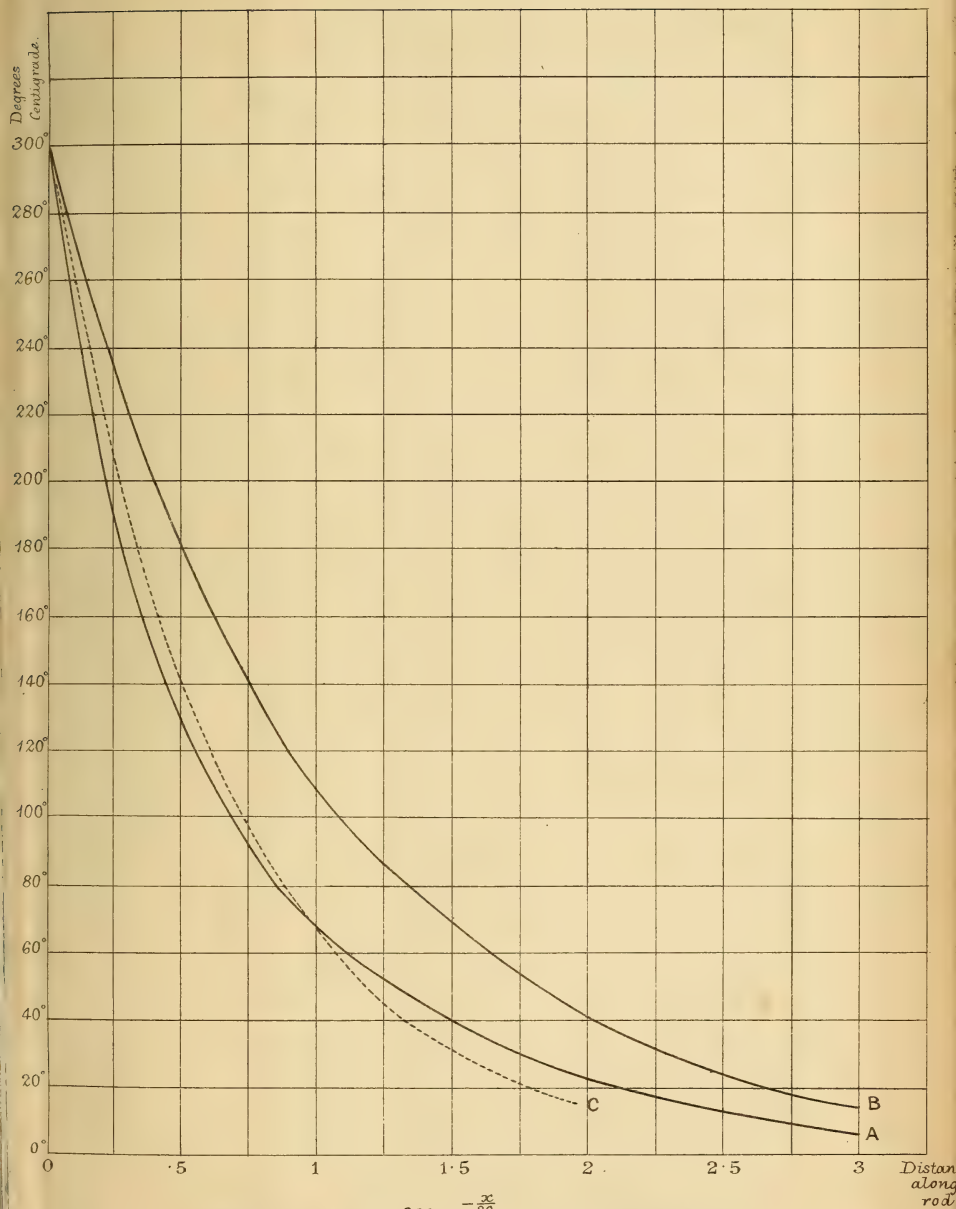


Fig. 3.
to nut size









Curve A is $y = \frac{300 e^{-\frac{x}{80}}}{2 - e^{-\frac{x}{80}}}$ see equation (20)

Curve B is $y = 300 e^{-\frac{x}{80}}$

Curve C is $y = 300 e^{-\frac{x}{53}}$ see § 21.

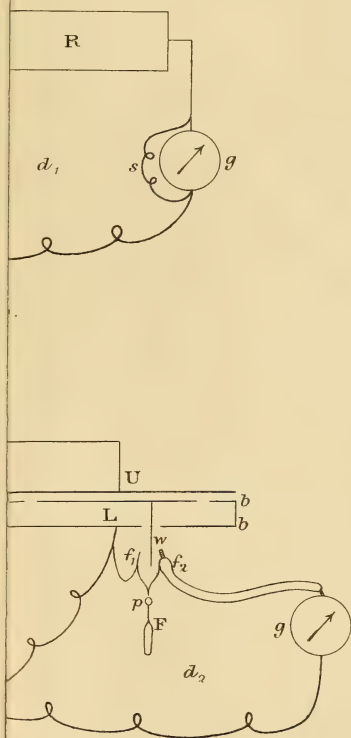
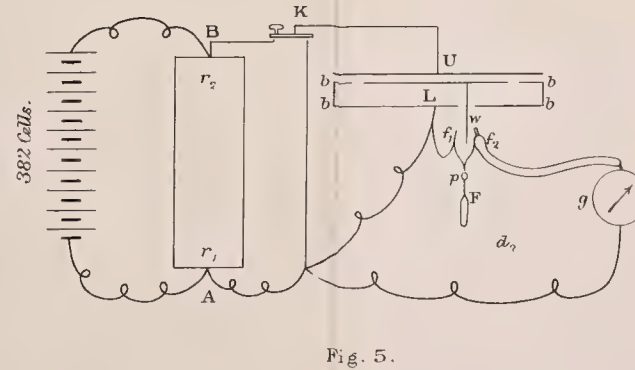
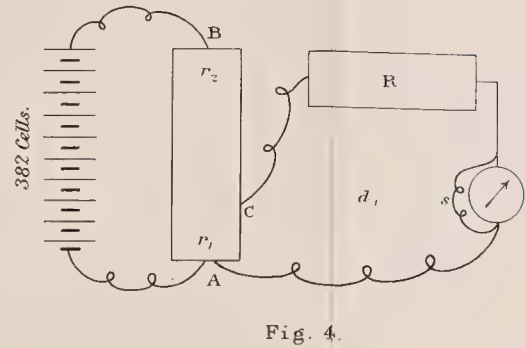
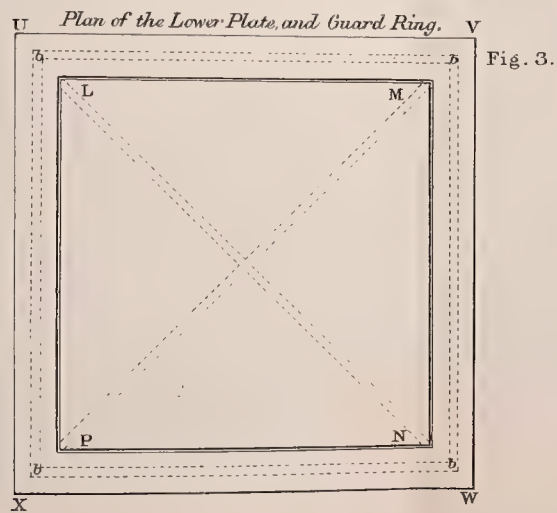
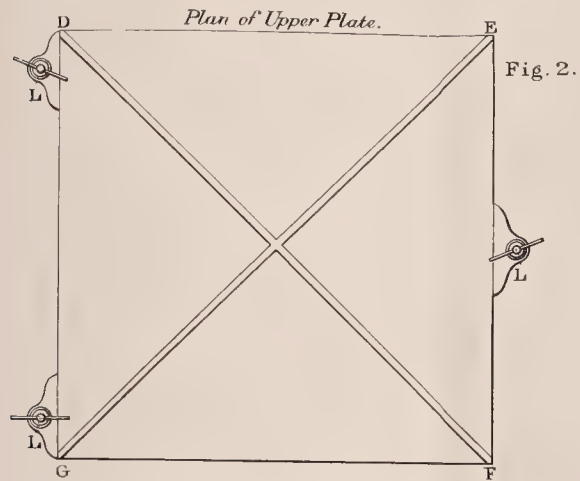
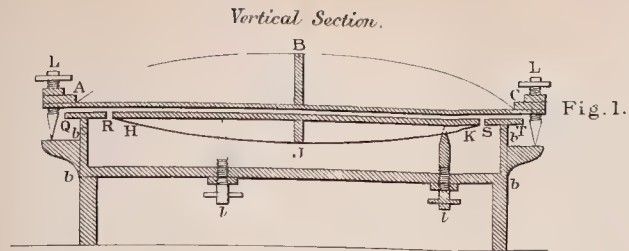


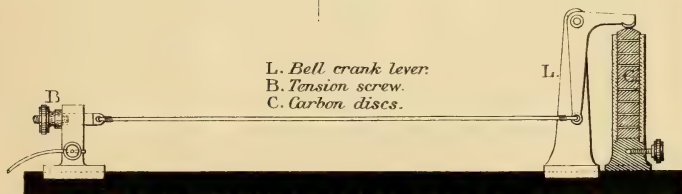
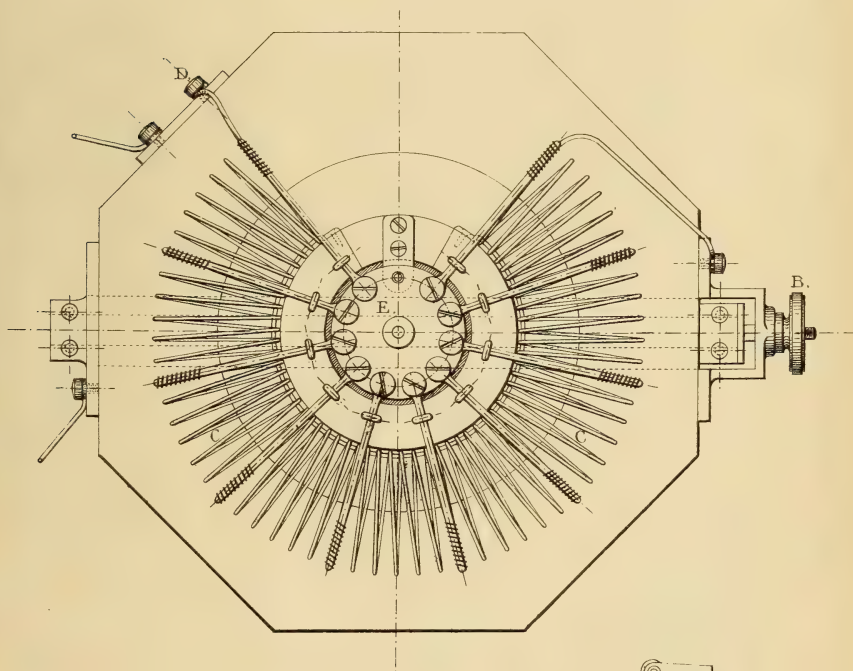
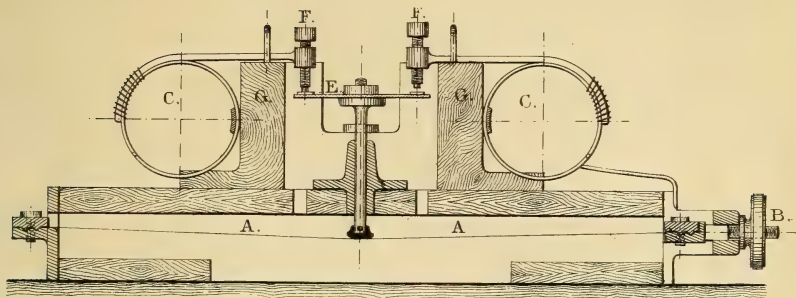
fig. 5.





CURRENT REGULATOR.

Scale $\frac{2}{5}$ of full size.



L. Bell crank lever.
B. Tension screw.
C. Carbon discs.

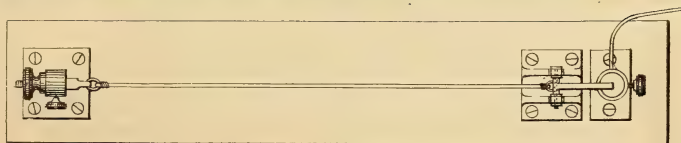




Fig. 1.

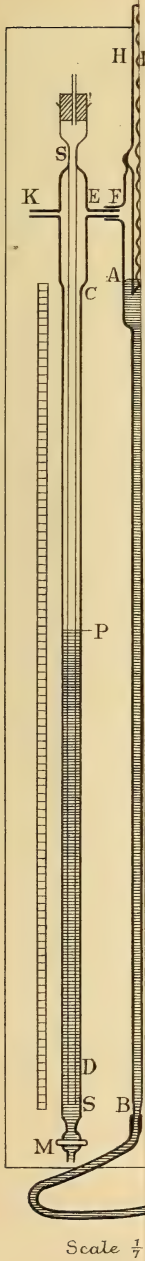


Fig. 4.

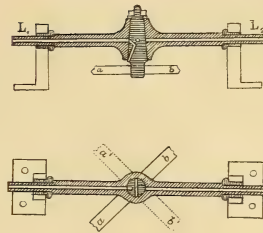


Fig. 5.

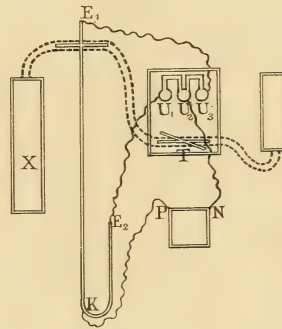


Fig. 3.

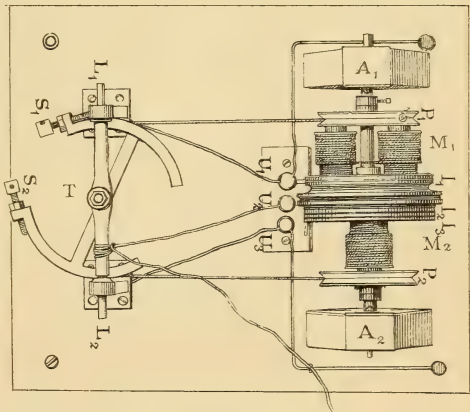
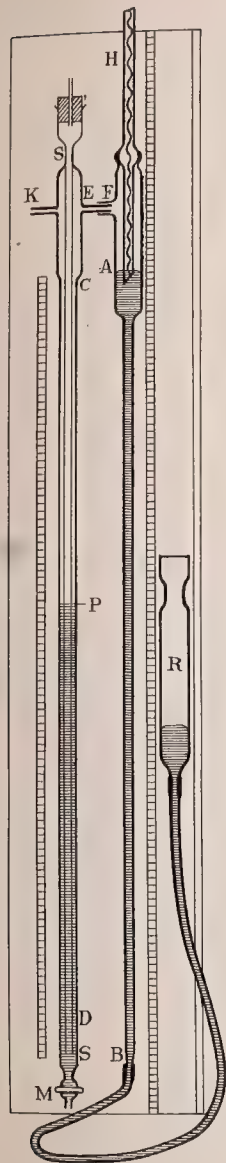
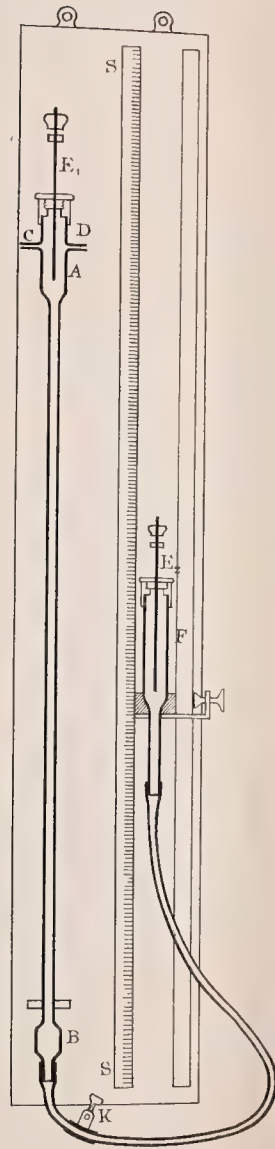


Fig. 1.



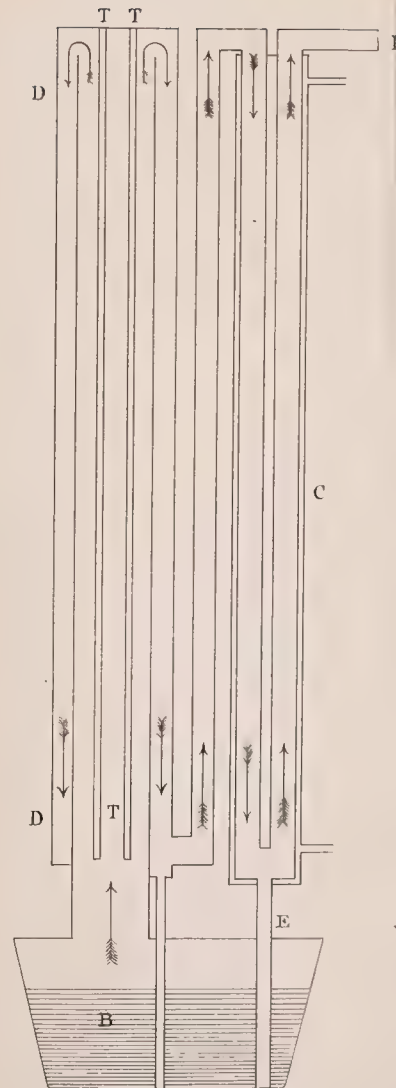
Scale $\frac{1}{7}$

Fig. 2.



Scale $\frac{1}{7}$

Fig. 6.



Scale $\frac{1}{7}$

Fig. 4.

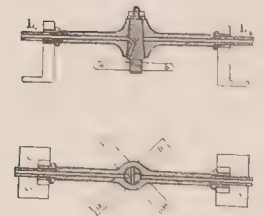


Fig. 5.

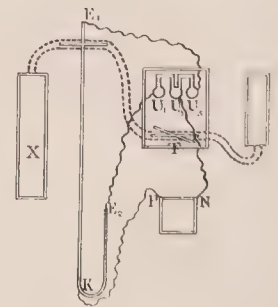
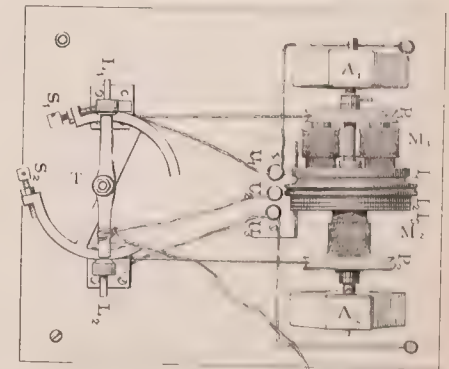


Fig. 3.



Scale $\frac{1}{7}$

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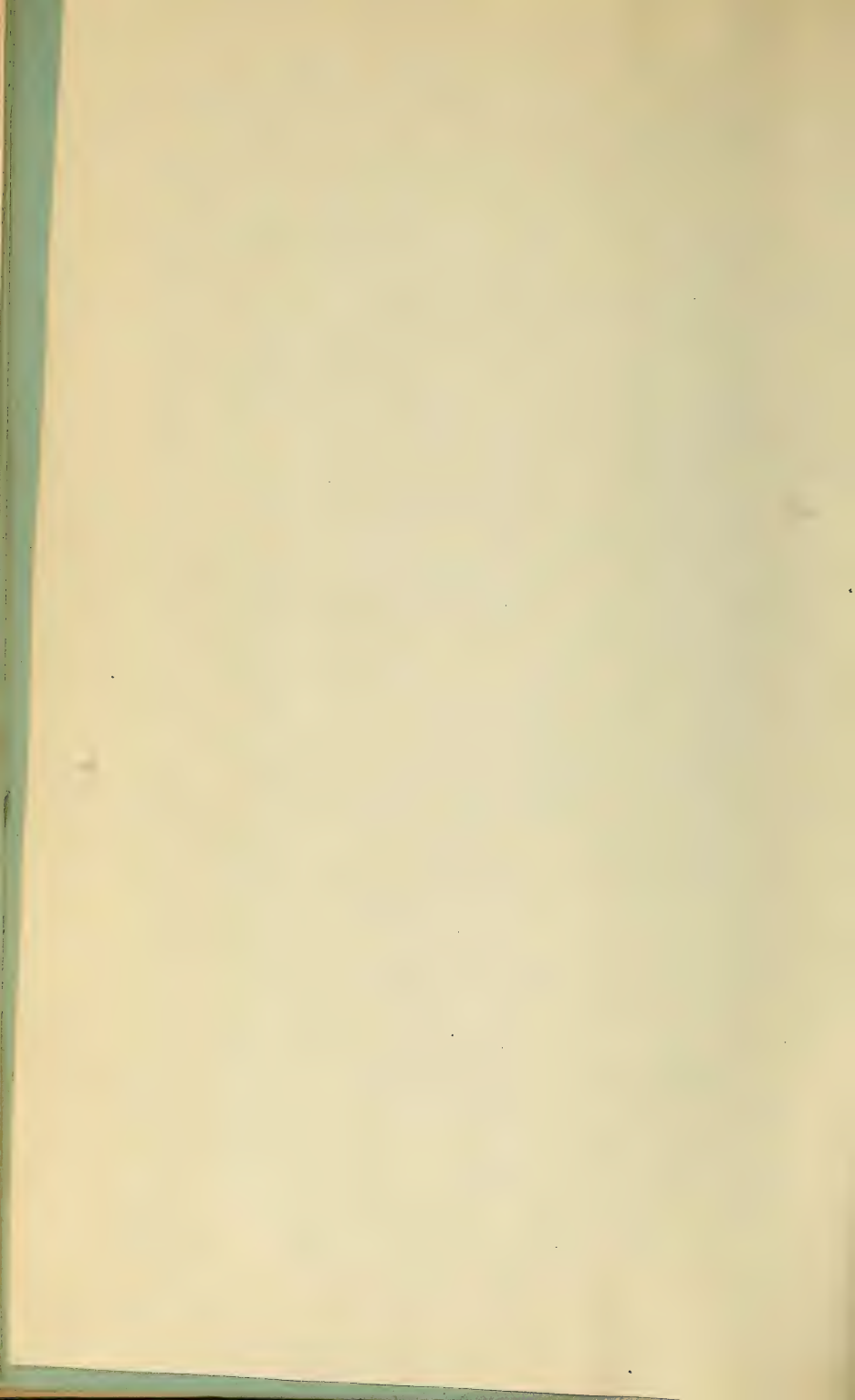
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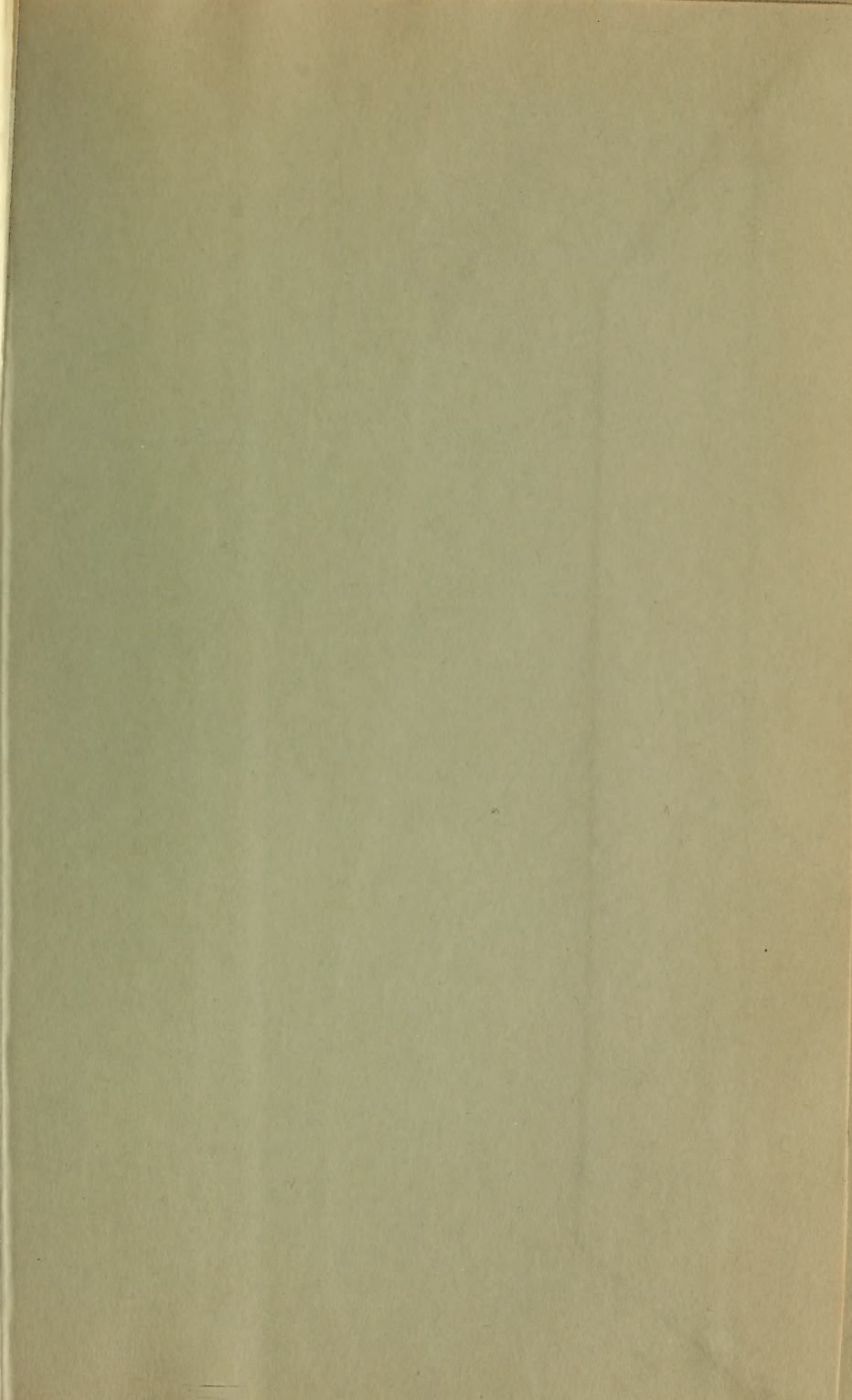
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